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*THE STUDY OF LITERATURE.**

BY W. P. ATKINSON.

I AM to begin with you to-day a course of instruction in English literature. What is it to be like? What is to be expected of you? What are you to expect from me? Why should any study of literature be required of you in a scientific college? These and the like questions no doubt occur to you, and it is the business of an introductory lecture to answer them; and it is the more important that I should begin by answering them, as my answers will probably prove quite at variance with a good many of the ideas you have brought into the room with you.

To begin, then, with my first question, What is my course to be like? I have to say at the outset that my subject is not going to be English literature, because English literature, at least as we are situated here, cannot be taught. This statement will perhaps surprise you. I suppose that when you studied geometry, your instructor hardly began by declaring that he was not going to teach geometry, because geometry could not be taught. He did teach you the body of axioms, demonstrations, and corollaries that constitute geometry, and you took them into your minds and memories; and it is to be hoped you have most of them there still. Now, if I were to undertake to teach you English literature in that way, I should have to take you to a vast library containing thousands of books, and say, "See, that is English literature. I am going to teach you all that." It is obviously impossible. You might as well sit down by the side of Lake Cochituate and try to drink it dry; or rather you might as well try to drink the Connecticut River dry;

* Part of an introductory lecture to the Junior Class.

for literature is not a standing lake, but a fresh and running stream, continually fed with new supplies from fountains that are inexhaustible.

But could we not study what is called a Compendium of English Literature,— one of those books where the whole subject is conveniently packed into a single volume? Yes, we might; but to study it would be no more studying English literature, than reading the Boston Directory would be seeing Boston. To know that Smith is a shopkeeper at No. 2000 Washington Street, or that your friend lives in Commonwealth Avenue, may be very convenient pieces of information if you want to buy a hat or call on your friend; but you do not call it knowing Boston, much less knowing your friend. So to know that a young man named Byron was born in such a year and died in such another, and wrote Childe Harold and other poems which are considered fine; or that a wise man named Bacon was born in 1561, and wrote a certain book called the Advancement of Learning,— this may be convenient and useful information; but to acquire ever so much of it is no more studying English literature, than to build a scaffolding would be to build a house. Some knowledge, more or less, of that kind, is doubtless a necessary scaffolding to your literary structure, but it can never take the place of the structure itself.

The proposition would be too absurd for argument, if it were not that so much of that sort of teaching goes on. I might venture to guess that some of you have been taught in that way, and have no very friendly feeling toward literature in consequence. Certainly, compendiums of literature are useful books when they are well made, and so are dictionaries; I advise you to own one; but the place for it is on your study-table, along with your dictionary and your tables of logarithms, and those other books of reference which are rather to be called tools than books, and to be used in just the same way. For what is a compendium? Why, just as the dictionary may be said to represent in a certain way all that ever was written in the words it contains, so the compendium represents for certain purposes that vast library I spoke of, which contains in its thousands of volumes all that was ever written by English authors. But to know the compendium is clearly not the same as knowing all that.

Such sort of study, then, not being the study of literature, and the proper study of literature being for us impossible, what are we to do about it? At first sight it would seem as if we could do nothing worth doing, for we can spend very few hours together, and I am aware that outside the lecture-room your other studies leave very little time for me. Let me say, then, that in the sense in which you have to do a great

deal for your mathematical or your engineering and other scientific instructors, I hardly expect anything of you at all. In that sense I say; I do not say that I expect nothing from you. What I do expect will presently appear. But with ever so good a will to work for me in the way in which you work for your scientific teachers, you could not do it, because your time and brain-power are of necessity pretty much monopolized by them. If there is one thing more than another I feel disposed to caution you against, it is over-study. Perhaps some of you do not require any such caution, but I am quite sure that others do; and I speak from dire experience. I was sent to college very young, as was the bad fashion of those days. I was studious and ambitious, and in those days very little attention was paid to hygiene or athletics. The consequence was that I broke down in health, and suffered from it for years after. I would not discourage any one from hard work, for hard intellectual work is very wholesome. But hard study is like hearty eating, which must be accurately measured by one's digestive powers. Cramming the mind is just as bad as stuffing the stomach, and you can injure your brains quite as easily as you can spoil your digestions, and in quite the same way.

I tell you fairly, then, that I have no hard work for you, in the sense in which you use the phrase in connection with your scientific studies. I have no tough problems for you to solve, no long calculations for you to work out, no difficult formulæ for you to memorize. Do not suppose, on that account, that the real study of literature involves no serious work. I could give you plenty of work, and hard work, too, if there were time for it. There is no greater mistake than to consider the study of literature a sort of amusement. Practical men, as they call themselves, are apt to imagine that it stands in relation to scientific and other *solid* studies much as ice-cream stands to roast beef as articles of food. There could not be a greater misconception. I do not say that the difficulties of literary study are of the same kind as those of scientific study. On the contrary, they are of a very different kind. What I shall hope to show you is, that literature, in its way, forms, to say the least, as important and as difficult a department of study as physical science, and that he who does not study literature seriously does not study it at all.

Literature is a serious study, and no one will profit by it who does not approach it in a serious spirit. The theory that literature is intellectual ice-cream, while physical science is intellectual roast beef, is the shallowest of all ways of looking at it. You do not expect to solve your engineering problems without hard thinking and ample preparation.

You have studied algebra, geometry, and I know not what besides, before you can even approach them. But there are many who cannot be made to understand how the study of literature can need any sort of effort. The only needful preparation for the study of Shakspeare, they think, is to buy a copy of Shakspeare. Where is the difficulty in understanding plays written in English? Does one require any algebra to prepare him for that? So he reads Shakspeare, and says he is a great dramatist, and privately thinks him a bore, and greatly prefers one of Marryat's novels, which he can relish. He had better be honest and tell the truth, for there is no more discredit in not appreciating great writers when you first attempt to read them, than there is in not understanding descriptive geometry at a glance; though there is something very discreditable in merely repeating, like a parrot, what other people say of them. "What is the character of Milton's Paradise Lost?" asks the examiner. "Very sublime," answers the examinee. He really knows about as much of Milton's sublimity as he knows of Choctaw; but he knows that is what the compendium says, and so he says it.

I liked much better the answer I found once in an examination-paper to the question, "Who wrote the 'Wealth of Nations'"? "Never heard of the book," wrote the candidate," but should think it might be David A. Wells." There was something funny in the idea of Mr. Wells, alive there down in Connecticut, writing one of the greatest books of the last century; but I liked the answer, because it was an honest one. The writer was no more ashamed of his want of knowledge, than was Dr. Johnson when the lady asked him how he came to put such a queer definition of the word *pastern* into the first edition of his dictionary. "Ignorance, Madam, pure ignorance," answered the sturdy Doctor. There was no reason why our candidate should know even the name of Adam Smith, and he did show some knowledge of Political Economy, to the extent, at least, of having heard the name of one of our ablest living writers on that subject. Let us always be intellectually honest with ourselves,—always discriminate between what we really know and what we only know by hearsay. There is but one way to know a writer, and that is to read him with understanding and appreciation, to have that impression made upon your mind which he intended to make; to know him by hearsay is to know that he makes that impression on other minds, though he never made it on yours. You know Bacon only so far as you have read and understood his books, and Milton only so far as you have really felt the grandeur or beauty of his poetry, and you may not, for the time being, be capable of that,—your mind may not be mature enough; we do not expect children to understand the Cal-

culus. Or you may not possess the intellectual gifts that qualify you to appreciate certain kinds of excellence, just as some people are born without any musical ear, and others without sense of color. I see, for instance, that some apparently good judges hold Shelley to be not only a true poet, but one of the very greatest of English poets. I cannot enjoy Shelley's poetry as much as they seem to. Some of this modern Shelley-worship seems to me exaggerated,— an overstrained reaction against previous neglect. But I hesitate to contradict these claims, because I feel that it may be that I am not poetical enough myself fully to appreciate him, just as I am not musical enough to enjoy the highest kind of music as much as I know musicians do. The only way to resolve my doubts would be to make a thorough re-study of every line that Shelley wrote.

Now, it must never be forgotten that this is the only *real* study of literature; all else is only studying *about* literature. To know that a man named Scott was born in such a year, and died in such another, and wrote novels, and to read scraps and fragments of them in school reading-books, is plainly not to know your Scott. But to get so engrossed in one of Scott's novels that you do not know when dinner-time comes, and to read them all over and over till you know every scene and every character in them,— that is studying literature to some purpose; and it is the only kind of literary study that does us any good. When I was a very small schoolboy my father lived in the country, and we had but few books — for in those days good books were not so plentiful and cheap as they are now,— but among them was a set of the first American edition of the Waverley Novels. For, though I am not old enough to remember the time when they were first published, I can remember when they were new books, and nobody was quite sure as to their authorship. And from hearing them talked about I felt a youngster's curiosity to read them, and after much importunity I got leave; and I read them through and through, and over and over, and I consider that it was no unimportant part of my real education. For in the first place they exercised my imagination in a way my dry school studies never did; and no one is really educated who has not in some way had his imaginative powers brought into play. And in the next place it gave me a relish for the study of history, which has ever since been one of the greatest pleasures of my life.

There was also in the book-closet a little, badly printed, uninviting set of Shakspeare. I can remember at that same tender age trying them, but not liking or understanding them; and it was not till long after that I got to feel any real interest in Shakspeare. But I have always felt

sorry that I had no one at that time to *teach* me Shakspeare,— I do not mean to tell me when he was born, or where he lived, or to put me through the notes of a *variorum* edition, but to awaken me to a partial sense of his greatness,— for Shakspeare has something for all ages.

Just so far, then, as you may have been interested readers of books not scientific, you may be said to have been already students of literature. The books may not have been of a very high order, and there was probably no method in your reading, and no object except the amusement of the passing moment; nevertheless, there is hardly any book that is readable at all that does not furnish some food for the mind, though certainly there are many whose food is unwholesome. But, barring unwholesome books, I am for having readers begin with what they relish; because reading is like food,— its nourishing power depends a good deal upon our appetite. We may begin with relishing very poor stuff, but it will be the best for us if it is the best we can appreciate. If we do not begin with reading what we care for, we shall never learn to care for anything we read. I feel no shame in remembering that I once copied choice sayings from Mr. Tupper's "Proverbial Philosophy," and thought Mr. Bailey's "Festus," when it first appeared, a very great poem, and greatly admired Bulwer's flashy novels. It is true Mr. Tupper is an ass, but his pompous platitudes have a show of wisdom; and Mr. Bailey's "Festus" is to a truly great poem much as a brass band to a symphony concert; and my Lord Bulwer Lytton is little better than a very clever and pretentious charlatan, whose sham philosophy and tinsel sentiment are to true thinking and real feeling much what Providence plated ware is to solid silver. But we must all begin somewhere; and the somewhere in literature would seem to be whatever we are really able to like and to understand.

There is a sort of match, you know, that will not light upon any surface on which you may please to rub it, but only upon a surface that is properly prepared. Our minds are like such matches,— they do not catch fire upon every book, and hardly do any two upon the same. The good reader will search till he finds *his* books. My illuminating book may not be the same as my neighbor's, though sometimes books appear that seem to set on fire a whole generation. When I was a young man just beginning to think for myself, the books of Carlyle and Emerson began to appear; and you who can read their biographies, and who are beginning to think for yourselves in a wholly different intellectual atmosphere, can hardly realize the effect on some of us of those two very remarkable, though wholly different men. They set us all on fire. Here was a new philosophy. We looked down with a contempt that

was only equaled by our ignorance upon all old fogies. We had a new gospel, and were forthwith going to make the world over. But it is a tough old world. That was almost half a century ago, and it wants a great deal of making over yet. But I am not disposed to regret even the youthful extravagances of those transcendental times. There was a noble inspiration in them; and inspiration is what we want from literature. Those books roused and took hold of us. All our other teaching was dead; but here were live teachers, who waked up our faculties and set us on living and thinking for ourselves. I do not say there was no extravagance in all this discipleship; on the contrary there was a great deal. We swore by the errors of our masters as heartily as by their truths, and they led us into mistakes, because they themselves were not infallible. But being themselves independent, they taught us self-reliance, too. With a larger experience of life, the mistakes gradually righted themselves, while the good that was in their influence remained. A young man may count himself lucky who in the days when he is beginning to think can really enter into the spirit of so noble and so pure a guide as Mr. Emerson; and I say this although I believe I can see nowadays clearly enough Mr. Emerson's limitations, and can very well understand why he hardly affects some readers at all, while others he affects only by way of repulsion.

You see I am dealing with literature here as a reality, something as real as a power-loom or a locomotive,— indeed, a good deal more real. But it is the hardest thing in the world for what are called practical men to look at it so. Solid things they can understand — granite and iron, and things made of granite and iron. These are their realities, but the world of books and poetry is to them the world of girls and schoolmasters, and dyspeptic ministers; in their hearts they are very apt to despise it. Tennyson's poetry, and the mining and mechanical laboratories, and that machine down stairs for breaking timber! Are you going to tell me that one is as real as the other — hearts and darts, and that sort of thing — very nice for young ladies and young gentlemen in love. Yet a good writer has recently declared that the state of English poetry is a matter of national concern — exactly as if he were writing about the currency. "Philosophy, sir," said a worthy man to me once with great emphasis — a clever man, too, in his way — "what they call metaphysics, sir, is a mass of unintelligible nonsense; it is nothing but words. The metaphysicians themselves don't know what they are talking about." He was quite unconscious that he was only proclaiming his own incapacity to judge. He would have stared if I had replied, "My dear sir, the very same is true of the Differential Calculus.

I have looked into it myself, and I assure you I could not understand a single line." This is the way of thinking that our present devotion to physical science tends to bring with it, and is what renders it so necessary that the study of literature should have some place in a scientific education. For I need not say that it is a very false and onesided way of thinking, and when carried to an extreme, involves us in all the grossness of materialism. For what, after all, are your granite and iron? You do not know. Your machine is an idea; it is not the particles of matter that constitute its reality. You can resolve it into a thought, and one that may not be far removed from poetry. The really great man of science is an idealist; it is the small and second-rate men of science who stop short in materialism. This apparent strife between materialism and idealism, between literature and science, is only the fable of the gold and silver shield over again. The two are only halves of one great whole, neither of them intelligible if you neglect the other, and meeting together if you only go high enough.

You must study literature, therefore, if only that you may not ignorantly despise it; for we are apt to despise what we do not understand. But I should like to know if you can possibly call yourselves educated so long as you wholly neglect those studies which alone enable you to understand yourselves and your fellow-men. Are you going to make yourselves engineers of railroads and know nothing of the engineering of life? For just as physical science deals with matter, organic or inorganic, so literature deals with man. You know the divisions of science well enough, Chemistry, Physics, Biology, and the rest, and their close interdependence, and how each is necessary to the other, because all are but branches of one great science,— the science of the laws of the physical universe in which we live. Well, in like manner the various branches of literature are but branches of the complementary science of man. History is the record of his doings; Political Science and Political Economy, the account of the development of his social nature. Ethics embodies his notions of right and wrong. Theology is the account he gives himself of his relations to God. Poetry is one of the outcomes of his sense of beauty. Whatever branch of literature you examine — and I include in the term literature here all books not belonging to physical science — you find it leads back to man as its centre, just as whenever you touch a branch of physical science you find it is concerned with some set of phenomena of the material universe. We are falling, in these days, into a very abusive employment of the term Science instead of Physical Science, as though there were no science save that of material things.

And now, to return once more to the method, some of you may well have thought that this reading only what you like is a rather too free-and-easy fashion of study. Am I quite sure, you ask, ever to learn to like the right thing? And it would be a very pertinent question. Here is where the guidance and instruction of a teacher may well come in, if it is only the right sort of guidance. But I should like to get out of those of you who were taught literature in school, just what you think of the results of your teaching. Some of it was doubtless very good, as the weekly papers I get from my classes show. But I remember a fellow here some years ago — if I may repeat a story I have told in a printed lecture*— who gave me a list of English classics as long as my arm which he said he had "studied" in school; and he wound up with saying, "And if there is any study I *hate* it is Rhetoric and Literature." I remember one of his classmates who said he had never been *taught* literature at all, but that he was the son of a country doctor, and his father, besides his medical library, had a pretty good collection of miscellaneous books, and that he had read a great many of his father's books in the retirement of his father's haymow. I need hardly say that the instruction of the haymow proved, in this case, better than the teaching of the school. It is a poor instruction that ends only in hatred of the subject taught. "Then farewell, Horace, whom I hated so," sings Byron of his classical education. My friend of the haymow, no doubt, for want of guidance, lost time in reading poor books; but if so it would only be another illustration of my thesis,— that as between a firstrate book which you cannot read with any relish and an inferior one which you can, you had better begin with the inferior one. Macaulay, for instance, does not stand very high as a historian with genuine students of history. He is prejudiced, partial, inaccurate. He is rather an essayist and rhetorician than a genuine writer of history. But he has one supreme merit — he is interesting. It must be a very dull reader who cannot read Macaulay. If, therefore, you cannot at first relish the histories of ponderous Bishop Stubbs, or learned Prof. Freeman, or dull and accurate Mr. Hallam, why, begin with Macaulay, begin with Froude, begin with Scott's novels. Learned Sir Francis Palgrave says that Scott's Ivanhoe, as a historical picture, is incorrect, and learned Mr. Kemble is of the same opinion; and Sir Francis and Mr. Kemble were the two men in the world best able to judge. But I am not going to be deterred from reading Ivanhoe by that. There is something better in history than mathematical accuracy, and that is *life*. Now, Scott has a power which the learned anti-

* "On the Right Use of Books."

quaries do not possess; he can make the past live again before us. We can pardon much incorrectness for the sake of that wonderful gift. There are other and higher kinds of truth than mere truth of detail. Learned Sir Francis himself tried his hand at a historical romance. I have no doubt it is strictly accurate, but it has one fault — it is unreadable.

I am very sure that a great many men have been prevented from ever becoming readers by the way they were taught at school. I once saw an essay in a medical journal "On the Artificial Production of Stupidity in Schools." Bad teaching can go a great way toward spoiling interesting subjects, as the best food can be spoiled by bad cookery. Put a patient into the hands of an ignorant doctor and though he might have survived if he had been let alone, the doctor will be sure to kill him. Thus native tastes and aptitudes are killed out of the mind by pedantic teaching, and our schools have to answer for much hatred of good reading. When I was a schoolboy we were not taught much in school except Latin and Greek, or rather Latin and Greek grammars, and mathematics. Those are dry subjects, and will bear a good deal of pedantic pounding. I have always felt it a great misfortune that I never got any good instruction in natural and physical science, but I am very glad they left me to read what I pleased, and to study history and literature in my own fashion. Not that good teaching would not have greatly helped me, but that bad teaching would have been worse than none. I am afraid matters are not much mended. One of the Harvard College professors of History has been making some investigations into the teaching of history in our secondary schools, and has published the results,* which are not very encouraging, and go far toward explaining certain queer entrance-examination papers I sometimes have to read. Professor Hart finds but few schools where history is properly taught. But a great change is coming over all our methods of teaching English subjects; and it must be acknowledged that both history and literature are difficult subjects to teach, and he will surely fail who can see no difference between the teaching of literature and the teaching of mathematics.

I said it was impossible to teach you literature: I did not say I could not try to teach you *how to study* literature. Imagine yourselves about to set out on a long journey, and that you have a friend who has been over it, or partially over it, before. He cannot go with you, but he can sit down with you and map out the way. He can explain the nature of the journey, the equipment you will require, the difficulties you will have

* In the September number of the *Academy*, a Journal of Secondary Education, "issued under the auspices of the Associated Academic Principals of the State of New York."

to encounter. He can point out the places best worth seeing, the long, sterile tracts that may be rapidly passed over, the bogs and fens where disease and death are always lurking, which you are to avoid. He can, perhaps, accompany you some small part of the way. And, above all, if he have the true traveler's spirit, he can infuse the same spirit into you who may have it not, and perhaps make of you in the end far better travelers than himself. Shrewd old Archbishop Whately says that teachers are only guideposts, who have to stand still and point the way, while others make the journey. So the teacher of literature, though a lifetime is not long enough to traverse the vast field of thought represented by that single word even in its narrowest meaning, can even in a single course of lectures do something toward setting you on your journey and showing you the way; and this is all the more important if, without some such guidance, the chances should be that nineteen in twenty of you will either start on a wrong road, or never set out at all.

My subject can be dealt with in a great variety of ways. I might in this single course of lessons run rapidly over the whole field of English literature, from Chaucer and Wyclif to Tennyson and Ruskin; but this would be unprofitable. You will find it better done than I could do it in the little Primer of Mr. Stopford Brooke. Sometime during the winter we will make a chronological table of periods, for it is necessary to know the relative position of great writers in time and in English history. That is the sort of knowledge one expects to get from compendiums, and which a little diligent use of such books will always impart. A better way for us would seem to be to select the group of writers belonging to some one period, and confine ourselves to that. We shall thus be able to examine it with some care, and in a way that will be applicable to other periods. It will still be only learning how. I could wish we might really study the period in question,— that is to say, read, or at least read *in*, all the books I shall have occasion to mention; but that will be impossible even for a single period, both for want of time, and for want of the reference-library, which is the working laboratory of the student of history and literature. The next best thing will be to take down all my references in your note-books, so that you may have the outline of that course of reading which would have constituted the study of the period if you had been able to devote an amount of time to it, somewhat in proportion to the time you have to devote to your chemistry or your engineering. Perhaps, with more leisure hereafter, you will be glad to avail yourselves of its guidance.* I shall be satisfied if

* I have much evidence to show that our graduates do make great use of their literary and historical note-books.

you are able to say to me what one of our graduates said to me the other day respecting a similar course in history: "You could not teach us much history, for you had no time. What you did do for me was to revolutionize all my ideas as to what history is, and how it ought to be studied."

What is the period best suited to our purpose? It must not be too remote, for then it would require too much historical and antiquarian knowledge to understand it. The period of Chaucer and Wyclif, for instance, is very interesting, but one must have much knowledge of mediævalism before one can really throw one's self back into the times of Edward III.; and without such historical knowledge it is useless to try to study literature. Even the times of Shakspeare and Elizabeth, or of Milton and the Commonwealth, are grown very remote to us in this nineteenth century. If, on the other hand, we take living writers and our own times, they are too near. We cannot judge impartially; we do not yet know who are our greatest writers. Time has not yet set his seal on their works, and many a flourishing reputation of to-day, in fifty or even in twenty years will have wholly withered away. But the same objection does not lie against the age that immediately preceded ours. It is a period very remarkable in itself, near enough to be intelligible and interesting, and yet far enough off to enable us to judge with some approach to accuracy, the relative value of its chief writers. The little celebrities of the day have almost all disappeared from sight, and have to be hunted for with a microscope; we will do some of that hunting before we have done, for these little great men of the past are often both instructive and amusing; while the really great writers of those days now begin to appear in their true proportions, and we can estimate their relative value and fix their permanent place with some approach to accuracy.

I shall try, then, to deal with the history and literature, the life and thought, of this particular period in so much detail as shall give it a living interest for us, and shall furnish an example of the method by which all other periods should be treated. Thierry has well said that the very life of history lies in its details. That is why there is no life in compendiums and abridgments,—why there is no short cut possible to a real knowledge of these things. We must put life into our lessons, because it is with live men and women that we are to deal. Next time I will introduce you to some of them in the shape of a group of boys.

THE EFFICIENCY OF THE BRUSH STORAGE BATTERY.

BY EDWARD E. HIGGINS, S.B.

Read at a meeting of the American Academy of Arts and Sciences, March 9, 1887.

IN the spring of 1885 the Massachusetts Institute of Technology received, through the courtesy of the Brush Electric Co., a storage battery consisting of twenty "ten-light" cells, so called, said to be new, in good condition, and "formed" ready for use.

The Brush cell of this size consists of two negative plates, each 8 inches square by $\frac{3}{8}$ inch thick, and one positive plate of the same area but of double thickness, the latter presenting an estimated working surface of about 900 square inches. The weight of the three plates is about 28 pounds. The battery is intended to be charged on a Brush arc light circuit, and to be discharged through ten Swan incandescent lamps. The normal currents of charge and discharge would be, therefore, approximately 10 ampères each.

The battery was in constant use during the month of May, but from June to October it lay idle, with the acid in the cells — a disposition very unfavorable to its longevity. From October to January a number of preliminary tests were made, which furnished valuable information for later use. In February, 1886, was commenced the series of tests forming the subject proper of this paper, the design being to determine the efficiency of the system under different conditions of charge and discharge, and to find the loss of charge on standing.

The battery, which was located near the dynamo-room of the Rogers Laboratory, was connected with a switch-board at the dynamos by heavily insulated copper rods. A Brush "two-light" machine, capable of giving from 1 to 100 volts, was generally used in charging; but in some of the tests, where currents larger than 11 ampères and smaller than 7 ampères were required, a Weston 60-light incandescent machine was employed. The battery was charged through a Brush "current manipulator,"—a device having several functions in the practical use of storage batteries, but employed by me only to prevent reversals of the field magnetism of the dynamo. The "manipulator" is arranged to charge at 10 ampères, but will perform its office with a current as low as 6 ampères.

The measuring instruments used throughout the tests were two of Sir Wm. Thomson's graded galvanometers, giving current and potential, and which were located in an apartment of the Physical Laboratory directly above the battery-room. A specially constructed mercury commutator

permitted reversals of current without breaking circuit, and variation of current was secured by means of a large upright frame wound with German-silver wire, capable of carrying 20 ampères. This frame was placed at a distance and in a position such that the current in the wires could have no appreciable effect upon the readings.

EFFICIENCY TESTS.

The commercial efficiency of a storage battery is represented by the ratio of the external electrical energy of discharge to the total energy entering the battery, the energy expended at any moment being obtained by multiplying the corresponding electro-motive force in volts at the battery terminals by the current in ampères passing in the circuit. In these tests it was decided to deal with the commercial efficiency only, as it indicates most nearly the practical value of the battery for the purposes intended.

To obtain the efficiency of a battery in any true sense, it is, of course, essential that the battery should be in exactly the same condition after the test as before. The problem then, is, to adopt a method such that the conditions may be accurately known, and capable of being regulated. After much experimental work two methods were adopted, either of which would seem to give reasonably correct results. In the first method the test was begun with the battery fully charged ; it was discharged through a resistance of German-silver wire at a current which, starting at a given value, gradually fell about ten per cent, to a point where the current and electro-motive force began to show a more pronounced falling off. Stopping the discharge at this point,— the limit to which it is advisable to discharge a battery in practice,— the dynamo was immediately connected and the charge commenced, generally with a current of about ten ampéres. The point of complete charge was indicated by a sudden marked rise of electro-motive force, due to the liberation of gas by electrolysis, and by the appearance of the gas in the form of a violent ebullition in the cells.

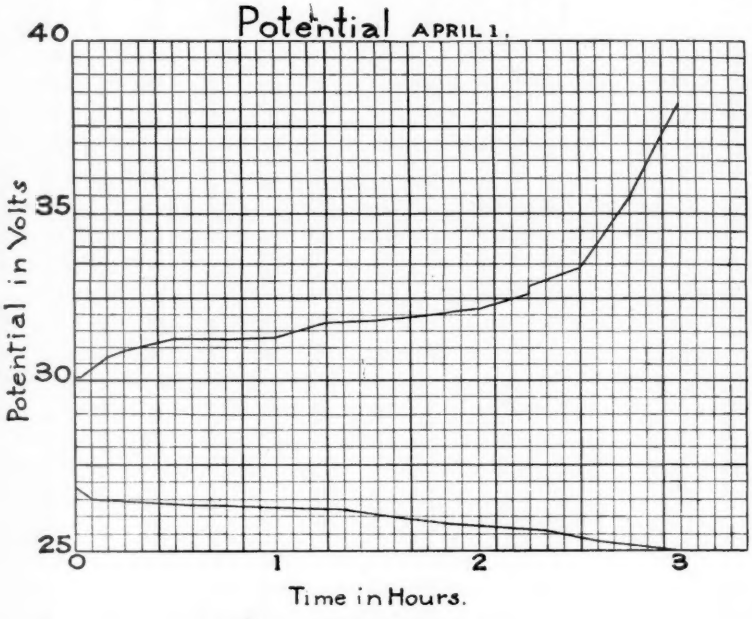
The second method adopted was similar to the first, the only difference being that the battery was first charged from the point of partial exhaustion, then discharged to the same point, care being taken by means of a short supplementary discharge to leave the battery in precisely the same condition as at first.

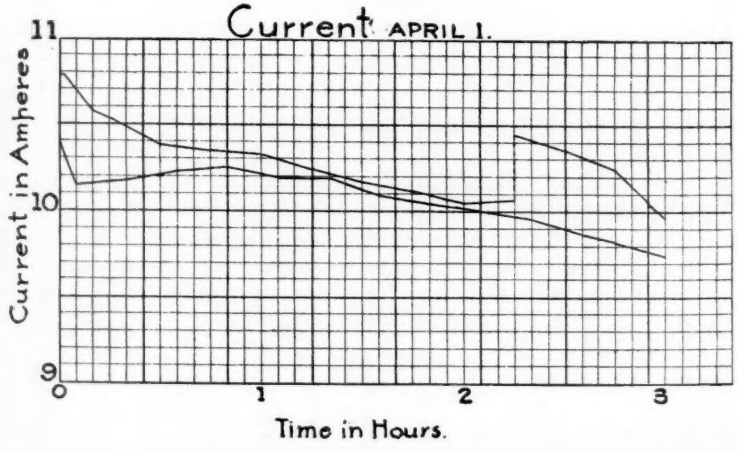
Measurements of current and electro-motive force were made at intervals of from ten to thirty minutes during the test, the sum of the products of current, electro-motive force, and time between observations, giving the total energy consumed.

TABLE I.—EFFICIENCY TESTS: SUMMARY. FIFTEEN-CELL BATTERY.

Date of Test.	CHARGE.						DISCHARGE.							
	Duration. H. M.	Highest E. M. F.	Lowest E. M. F.	Highest Current.	Lowest Current.	Energy. Volt-ampère Minutes.	Duration. H. M.	Highest E. M. F.	Lowest E. M. F.	Highest Current.	Lowest Current.	Energy. Volt-ampère Minutes.	Order	Efficiency %.
March 18	3.20	37.09	30.91	10.99	9.05	67299	3.30	26.37	24.86	10.60	9.63	54356	D.C.	81.3
March 24	5.29	37.98	31.87	11.88	9.46	109364	8.58	27.13	25.41	5.57	5.26	77409	C.D.	70.8
March 29	3.08	37.98	30.84	11.30	9.82	66115	4.23	26.79	25.41	8.14	7.51	53448	C.D.	80.8
April 1	2.58	38.19	30.08	10.78	9.96	59230	2.58	26.31	25.07	10.36	9.66	45988	D.C.	77.6
April 1	2.58	38.19	30.08	10.78	9.96	59230	3.	26.51	25.	10.40	9.75	47105	C.D.	79.6
April 7	3.	38.19	30.22	10.78	9.88	60998	5.30	26.86	26.10	5.63	5.47	48884	D.C.	80.3
April 9	1.32	38.46	31.66	20.12	13.63	54159	1.41	25.55	23.97	16.12	15.06	41529	D.C.	76.7
April 15	4.20	38.05	30.77	12.10	10.71	89005	4.33	26.17	23.97	10.47	9.57	70293	D.C.	78.9

On February 17th, five cells which previous tests had shown to be in an electrical condition somewhat different from the others, were removed from the 20-cell battery. The remaining fifteen cells were tolerably uniform as to electro-motive force, time required to charge, etc., and were connected up in a battery upon which a series of tests of





efficiency, storage capacity, etc., were made. A summary only of these tests, embodying the principal characteristics, is reproduced here in tabular form (Table I.). The details of one complete test with accompanying curves are also given, to illustrate the method pursued (Table II.). The upper curves correspond to the charge, the lower ones to

TABLE II.— EFFICIENCY TEST, FIFTEEN CELLS, APRIL 1.

CHARGE.					DISCHARGE.				
T.	C.	E.	C. E.	C.E.T.	T.	C.	E.	C. E.	C.E.T.
12.02	10.78	30.08	324.3		3.10	10.40	26.92	280.00	
12.10	10.58	30.70	324.8	2596	3.15	10.15	26.51	269.1	1373
12.15	10.54	30.91	325.8	1626	3.30	10.18	26.44	269.2	4036
12.30	10.39	31.25	324.7	4878	3.45	10.23	26.37	269.8	4042
12.45	10.35	31.25	323.4	4860	4.00	10.25	26.31	269.7	4047
1.00	10.32	31.32	323.2	4849	4.05	10.20	26.24	267.6	4029
1.15	10.25	31.73	325.2	4863	4.30	10.20	26.17	266.9	4008
1.30	10.18	31.87	324.4	4872	4.45	10.10	26.03	262.9	3973
1.45	10.13	32.00	324.2	4864	5.00	10.04	25.82	259.2	3915
2.00	10.06	32.14	323.3	4855	5.30	9.96	25.62	255.2	7716
2.15	10.08	32.62	328.8	4891	5.45	9.88	25.34	250.3	3790
2.15	10.46	32.83	343.4		6.00	9.80	25.14	246.4	3726
2.30	10.37	33.38	346.1	5170	6.10	9.75	25.00	243.7	2450
2.45	10.25	35.51	364.	5325					
3.00	9.96	38.19	380.4	5581					

Energy of charge, 59230 V. A. M. Energy of discharge, 47105 V. A. M. Efficiency, 79.6 per cent.

the discharge. The breaks in the curves of charge indicate a slight accidental sudden rise of potential and current such as frequently occurs in these operations.

These tests seem to show that the efficiency of the battery with different charging and discharging currents does not vary greatly through quite a large range.

STORAGE TESTS.

Three tests of the 15-cell battery were made, for the purpose of detecting leakage while standing. In the first, the battery was fully charged and left standing for 45 hours, at the end of which time it was discharged for three hours, then charged as in an efficiency test. The fall of potential and current was practically the same as in previous tests, and little leakage could be detected. The "efficiency" obtained in this manner was from 75 per cent to 80 per cent, as against 77.5 per cent and 75.7 per cent obtained the same week.

The second and third tests were simply discharges, one being taken after standing 45 hours, the other after one week. The curve of discharge in the former test was of such a nature that it could be directly compared with that of a previous efficiency test, simultaneous values of current and electro-motive force at a certain place in each curve corresponding very closely. At this point, then, the battery was in the same condition in both cases. The total energy taken out up to that point was in the efficiency test, 37139 volt-ampère minutes, and in the storage test, 35613 volt-ampère minutes, showing an apparent loss of about 4 per cent. Consecutive discharge curves, however, often vary as much as this, even from a full charge, so that little reliance can be placed on this result. The leakage is certainly small.

The one week storage test showed rather more signs of leakage, as was, of course, to be expected; but there seems to be no immediately previous curve with which it may be directly compared, and it is to be regretted that the test was not completed by charging and finding the energy ratio as in an efficiency test.

Summarizing the three tests, it may be said that the leakage appeared to be very small. Leakage, however, tends to take place at the glass rods supporting the plates, which are placed directly over the acid in the cells, where evaporation, ebullition, etc., can hardly fail to cover the surface— itself highly hygroscopic— with a conducting film of moisture. It was intended to carry out further experiments in this direction, not only on the battery as it stood, but also after altering the method of supporting the plates; but want of time rendered this impossible.

ROGERS LABORATORY OF PHYSICS, November, 1886.

COMPARISON OF METHODS OF TESTING INDIGO.

HELEN COOLEY, S.B.

INDIGO analysis presents one of the most difficult problems of analytical chemistry. Out of the great number of methods of analysis proposed, those have been chosen for closer examination which have found acceptance among technical and analytical chemists. As experience has shown that methods which yield excellent results upon certain grades of indigo fail to give satisfaction when applied to other grades, it was thought best to test each method upon several grades. For this purpose ground samples of the following well-known varieties were prepared, viz.; Watson's Best, Bengal Blue, Kurpah Blue, Bengal Red, Oude, Guatemala, and Kurpah Red. As a standard, a quantity of indigotine was prepared by the action of sodium xanthate upon the commercial ortho-nitro-phenyl propiolic acid[1], purified by crystallization from aniline[2], and was finally sublimed. The analysis then gave the following result: —

	Found. I.	II.	Theory for $C_{16}H_{10}N_2O_2$.
Nitrogen,	10.68	11.00	10.68

SPECIFIC GRAVITY.

Leuchs[3] maintains that a relation exists between the per cent. of indigotine and the specific gravity of an indigo. In general, the highest

	Indigotine by Fritsche's Method.	Specific Gravity.
Kurpah Red	45.28	1.529
Guatemala	47.04	1.559
Oude	52.90	1.427
Bengal Red	54.03	1.391
Kurpah Blue	55.11	1.129
Bengal Blue	57.60	1.431
Watson's Best	59.53	1.292

[1] Baeyer, Deutsches Reichs Patente, 11857, 11858, 14997.
[2] Aguiar and Bayer Ann. d. Chem., 157, 366.
[3] Jour. Pr. Chem. N.F. 4, 349.

grade of indigo has the lowest specific gravity; but exceptions to this general rule are very numerous. Certain medium grades with a high per cent. of indigotine are very heavy; and where judgment is to be given concerning several samples of the same grade, this test is without value, as will be seen from the preceding table.

ESTIMATION OF THE ASH.

This estimation is commonly made in indigo testing, but is usually without the slightest value. In the following cases the ash was obtained by burning the indigo in a porcelain boat in a muffle.

	Indigotine.	Ash.
Kurpah Red	45.28	21.20
Guatemala	47.04	14.49
Oude	52.90	7.02
Bengal Red	54.03	6.41
Kurpah Blue	55.11	17.54
Bengal Blue	57.60	7.50
Watson's Best	59.53	6.50

DYEING TESTS.

On Cotton.— These tests were made with small vats, and in all cases zinc vats were used. Two grams of indigo were rubbed to a paste with water, and then washed into a large beaker. One gram of zinc dust and one gram of lime, in the form of milk of lime, was added, and the volume made up to 600 c.c. Pieces of cloth of equal weight were used for each sample, and the dyeing, squeezing, and exposure to the air, and also the temperature of the bath, were the same in all cases. The depth of color found was not in proportion to the amount of indigotine present in the various samples. The unreliability of the method is shown by the fact that, taking four samples of indigo, duplicate tests will almost always fail to show even the same gradation among the samples. It is, of course, true that no better method of testing indigo exists than the dyeing of large quantities of cotton, 5,000 pounds for example, with various samples, but satisfactory dyeing tests cannot be made upon swatches of cotton.

On Wool.— The conversion of the indigo into the disulphonic acid, by the action of strong sulphuric acid, and the coloring of wool with this

acid, gives better results, although they leave much to be desired. Five tenths grams of indigo were digested with six grams of strong sulphuric acid in a warm place for twenty-four hours. The sulphonic acid thus formed was diluted to 1,000 c.c., and 10 c.c. taken for each swatch. The swatches weighed five grams, and were mordanted with five per cent. of sodium sulphate and three per cent. of sulphuric acid. They were then dyed under the same conditions, in every respect. The results give a rough idea of the value of the samples, but are not accurate enough to be of any considerable service.

LEE'S METHOD.

The idea of determining the per cent. of indigotine present in commercial indigo by removing it by sublimation and weighing the residue, is an old one. C. Tennant Lee[1] has given this method practical value by heating the indigo (0.2500 grams of dry indigo) in a platinum tray, upon which it can be very thinly spread. This tray is 7 c.m. long, 2 c.m. wide, and 1 m.m. deep. The tray must be heated on an iron plate, and the temperature raised very gradually. A piece of Russia iron, bent in the form of an arch, is placed over the tray at a distance of 1 c.m. Lee's instructions were followed with the utmost exactness.

	Indigotine by Fritsche's Method.	Lee's Method.
Kurpah Red	45.28	45.40
Guatemala	47.04	47.03
Oude	52.90	52.58
Bengal Red	54.03	55.99
Kurpah Blue	55.11	56.34
Bengal Blue	57.60	57.60–57.47
Watson's Best	59.53	59.42
Indigotine	99.71	95.75–94.02

These results show that, in general, Lee's method gives very satisfactory results. There may be an error from the carbonization of indigotine giving too low results, and also from the decomposition of the organic matter present giving too high results. The method is rapid, never requiring more than one hour.

[1] Chem. News, 50, 49.

FRITSCHE'S METHOD.

Fritsche[1] reduces the indigotine to indigo white, and then precipitates and weighs the indigotine in an aliquot part of the solution of indigo white. This method of analysis was executed in the following manner: flasks were graduated containing 525 c.c., and fitted with rubber stoppers with two perforations; a short, bent tube passed through one hole, and a tube reaching nearly to the bottom of the flask through the other. These were fitted with rubber tubes and clamps. One gram of very finely powdered indigo was placed in the flask, 20 c.c. of potassium hydrate solution containing 40 per cent of K O H, and five grams of grape sugar dissolved in a little water, were added, and the flask was filled nearly to the mark with alcohol. The stopper was then inserted, the long tube was raised nearly to the cork, and the tubes left open. The flask was warmed gently for thirty minutes on a water-bath. The tubes were then closed, and the flask allowed to stand over night. In the morning the flask, which now contained an alcoholic solution of indigo white, was gently heated until the liquid reached the graduation. The long tube was now lowered, the short tube attached to a hydrogen generator, and portions of 100 c.c forced out. The portion of 100 c.c. was transferred to a beaker and a blast of air blown through, and by this the indigotine was precipitated in a crystalline form. The indigotine was filtered upon a weighed filter previously prepared by passing a similar solution of potassium hydrate and glucose in alcohol through it, dried at 110°, and weighed. This method is not accurate in inexperienced hands, but gives good results after practice. The results obtained by this method, after practice upon a single variety, are as follows:—

	Per cent. of Indigotine.	
Oude	52.86	52.78
Bengal Red	53.54	54.03
Bengal Blue	56.46	57.65
Kurpah Blue	55.11	55.32
Kurpah Red	45.28	
Guatemala	47.04	47.16
Watson's	59.51	59.59
Indigotine	98.50	99.71

[1] Ann. d. Chem., 44, 290.

These results check much better than those that were obtained by any other method, and are undoubtedly very close to the truth.

NORTON'S METHOD.

This method was first described, in the form used, by L. M. Norton, in 1880.[1] By this method the indigotine is reduced to indigo white, and the solution of indigo white added to a ferric solution. The amount of ferrous salt formed by the reducing action of the indigo white is then determined by titration with an oxidizing agent. One gram of finely powdered indigo was placed in an agate mortar, and rubbed with milk of lime. The paste was washed into a flask prepared as for Fritsche's method, one gram of zinc dust added, and the flask filled to the mark with water. It was heated for thirty minutes on a water-bath, closed, and allowed to stand over night. It was thoroughly shaken in the morning, allowed to settle, and portions of 100 c.c. forced off by hydrogen as in Fritsche's method, and poured into an iron alum solution. The ferrous salt formed was then titrated with potassium bichromate solution. The manipulation is much less difficult than with Fritsche's method, and the time required is less. Some practice is necessary to obtain good results.

	Fritsche's Method.	Norton's Method.
Kurpah Red	45.28	45.40
Guatemala	47.04–47.16	47.03
Oude	52.86–52.78	52.58
Bengal Red	53.54–54.03	55.99
Kurpah Blue	55.11–55.32	53.34
Bengal Blue	56.46–57.65	57.47–57.6
Watson's Best	59.51–59.59	59.42
Indigotine	98.50–99.71	94.02–95.75

The method gives satisfactory results.

McKINLEY'S METHOD.

The oxidation methods all depend upon the destruction of sulphindigotic acid by a known quantity of an oxidizing agent, and the consequent disappearance of a blue color in the solution thus treated. McKinley[2]

[1] Proc. Amer. Asso. Adv. Science, 1880, 358.
[2] Chemical News, 8, 284.

dissolves three tenths of a gram of indigotine in eight parts by weight of fuming sulphuric acid by heating, at 55° C., in a small glass-stoppered bottle, for from eight to ten hours. The solution is then washed into a porcelain dish, diluted to 500 c.c., and an oxalic acid solution containing five times the weight of the indigo added. It is then warmed, and titrated with bichromate of potash solution of about five grams to the liter. The blue color passes through green to an olive, and the end point is easily recognized with pure indigotine. The pure indigotine was used for standardizing the bichromate, and the samples dissolved and titrated in exactly the manner described above. The end point was more difficult to recognize. Certain samples can be tested by this method, but in general the results are unreliable, as the following table shows:—

	McKinley's Method.	Fritsche's Method.
Oude	52.42–52.58	52.86
Bengal Red	62.16	53.54–54.03
Bengal Blue	65.54–63.10	56.4–57.6
Kurpah Blue	66.72–57.86	55.11–55.32
Guatemala	62.56	47.12–47.04

ULLGREN'S METHOD.

Ullgren[1] makes his solution exactly as McKinley recommends. This indigo solution is made up to one liter, one hundred cubic cent. taken, placed in a porcelain dish, freely diluted, sodium carbonate added in excess, and this solution titrated with potassium ferricyanide solution containing three grams to the liter. Pure indigotine was used for standardization, and the indigo samples treated similarly. With a rapid titration the results are from fifteen to twenty per cent. too high. By regulating the time allowed for each addition of ferricyanide to take effect, the skillful operator can obtain any desired result. The Oude indigo gave from 54.5 to 80.0 per cent. of indigo blue.

RAWSON'S METHOD.

Rawson introduced his method[2] to meet the difficulties caused by the oxidation of organic matter other than indigotine. One gram of ground indigo was mixed with powdered glass in an agate mortar, and added gradually to 20 c.c. of concentrated sulphuric acid in a large porcelain crucible.

[1] Jour. Chem. Soc., 1865, 217.

[2] Chemical News, 51, 255. Dyer and Calico Printer, April, 1887.

The crucible was allowed to stand for two hours at 90° C.; the solution was then made up to one liter, and filtered. Thirty grams of sodium chloride were added to 50 c.c. of the filtrate. This causes a precipitation of the sulphindigotic acid as the sodium salt. This was filtered off and washed with 50 c.c. of concentrated salt solution, and then dissolved on the filter in hot water, cooled, 1 c.c. of sulphuric acid added, and the solution titrated with potassium permanganate solution. The permanganate was standardized with pure indigotine. Rawson makes an allowance of .0008 gr. of indigotine for the amount remaining dissolved in the salt solution. This is sufficient in the case of indigotine, but certain indigos were found from whose solution salt refused to precipitate the sulphindigotic acid with any completeness. In spite of this fact the method gives generally too high results, and the precipitation of the sodium salt cannot be accomplished without bringing down other organic matter.

	Rawson's Method.	Fritsche's Method.
Bengal Red	64.10	53.54–54.03
Bengal Blue	60.89	56.46–57.65
Oude	51.87–51.9	52.86–52.78

SPECTROPHOTOMETRIC METHOD.

This method, described by C. H. Wolff[1] and others[2], was examined, and the instrument used exactly as is described. The indigo (0.5 gr.) was dissolved in 5.0 gr. of sulphuric acid of 66° B., by allowing it to stand in

	Spectrophotometer.	Fritsche's Method.
Kurpah Red	25.8–30.20	45.28
Guatemala	28.3–40.1	47.04–47.16
Oude	22.2–28.3	52.86–52.78
Bengal Red	39.6–51.1	53.54–54.03
Kurpah Blue	51.5–55.6	55.11–55.32
Bengal Blue	56.9–57.55	56.46–57.65
Watson's Best	58.5–58.8	59.51–59.59

[1] Fres. Zeit., 1878, 65. [2] Vierordt and Hufner.

a warm place for twenty-four hours. The solution was then diluted to 1 liter, and 25 c.c. taken and again diluted to 1 liter, and this solution examined in the spectrophotometer. The results were far too low, and indicated an incomplete decomposition of the indigo. Stronger acid, finally equal parts of strong sulphuric acid, and Nordhausen acid were used, but without any marked improvement. The method gives fair results upon high grades, but low-grade indigos cannot be examined by this method.

SUMMARY.

In order to compare the results obtained by the different methods on the same sample, and to show the working of the methods, the following table is presented. It shows the average per cent. obtained by the different methods, as given above.

METHODS.

	Sp. Gr.	Ash.	Lee.	Fritsche.	Norton.	McKinley.	Ullgren.	Rawson.	Spectrophotometer.
Kurpah Red.	1.529	21.20	45.40	45.28	45.48	. . . . .	. . . .	. . . .	25.–30.
Guatemala.	1.559	14.49	47.03	47.10	47.14	62.56	. . . .	. . . .	28.–40.
Oude.	1.427	7.02	52.58	52.72	51.53	52.50	54.–80.	51.88	22.–28.
Bengal Red.	1.391	6.41	55.99	53.79	53.13	62.16	. . . .	64.10	39.–51.
Kurpah Blue.	1.129	17.54	56.34	55.21	55.23	62.	. . . .	. . . .	51.–55.
Bengal Blue.	1.431	7.50	57.54	57.06	57.27	64.27	. . . .	60.89	57.23
Watson's Best.	1.292	6.50	59.42	59.55	59.32	. . . . .	. . . .	. . . .	58.65
Indigotine.	. . .	. . .	94.88	99.10	98.44	. . . . .	. . . .	. . . .	

It is evident from this table that certain samples of indigo offer much greater analytical difficulties than others. It is not improbable that the claim of merit for certain analytical processes may have come from tests made upon a single sample of indigo. If McKinley's and Rawson's methods were to be judged by the results obtained in the analysis of the Oude indigo, they would be pronounced excellent methods. The preference is to be given to the methods of Fritsche, Norton, and of Lee. Either is reliable in practiced hands. The failure of the oxidation methods is due to the fact that the oxidizing agents act not only upon the indigotine, but upon other organic matter. An easy and reliable method, which can be employed for indigo analysis by the general analyst, is much to be desired.

Mass. Institute of Technology.

DISCUSSION OF THE PRECISION OF MEASUREMENTS.

BY SILAS W. HOLMAN.

IN quantitative measurement of any kind, and more obviously in physical work, but little progress can be made before questions respecting the precision of the results, and of the component measurements which go to make up those results, thrust themselves upon the observer. For simple, direct measurements, it may suffice to state in a few words the process of averaging and of inspecting the concordance of the measurements. These, with proper allusions to instrumental errors and their elimination, would at least readily satisfy the demands of all but quite advanced students. But, in fact, few physical, and almost no electrical measurements are direct, if we except such commercial work as corresponds in grade to the measurement of cloth with a yard-stick. For instance, a student is set to determine the specific electrical resistance of a sample of wire. He has, we will say, to measure the length, weight, and resistance of the wire, its temperature, and that of the resistance-box by which he measures. The thoughtful student will ask himself at once how closely each of these quantities must be measured, in order that he may get the final result with the desired closeness. He will appreciate, especially if he has previously done physical or chemical work, that not only must all the quantities be measured with some care, but that each will have its own special requirement as to grade of accuracy, which ought to be known in advance of his final observations, and which *must* be complied with if the desired final accuracy is to be reached. Thus in careful work, even when not of a high grade, and in qualitative as well as in quantitative study, it is essential to predetermine the necessary conditions in the component parts of the work; that is, to arrange or select the instruments for giving the most favorable conditions for the work, to proportion aright the parts of the instruments, to distribute properly the precision of the component measurements. And finally, less important in reality but ordinarily made more conspicuous, we should know how to form a numerical estimate of the reliability of the final result from the precision and nature of the component measurements which go to make that up.

The lecture course, of which the following pages contain the chief portions of the notes, was arranged with such thoughts as the foregoing in mind. Its direct object is to present to students somewhat advanced in electrical measurements, a systematic method by which such inquiries might be investigated; to illustrate this method by a number of original investigations worked out in detail; and to present some problems for solution.

The demands of the course did not render an exhaustive treatment imperative, so that the notes have not always been thrown into the most general form. But it is thought that they contain enough to serve as a basis upon which a competent student may solve any questions of this nature which he may ordinarily meet. The course assumes a knowledge of the differential calculus, and some acquaintance with the theory of probabilities, and the principle of least squares — subjects which in the order of these topics at the Institute precede the present one.

Given the result of an investigation or measurement of any kind, the first inquiry in regard to its quality is, or ought to be, what degree of confidence can be placed in it. In general, of course, the true value is unknown, and the above query can be answered only by statement of the probable accuracy of the result — for which it is necessary to find some numerical measure. It means nothing to the careful worker to be told that a given numerical result is "very accurate," or "quite accurate," etc., because he is aware that these terms have to different persons wholly different meanings. A numerical measure is essential. How can this be found?

It is obvious that an answer may be framed in this way: Let the same quantity be measured many times, by many wholly distinct processes, and by many independent observers. The average, or arithmetical mean of all these results will be likely to approach the truth, and to come closer to it as the number increases. For we may assume, with a high probability, that exactly the same sources, magnitudes, and directions of error will not occur uniformly in all the results, but that there will probably be many errors, some +, some —, and of graded magnitudes, which, in the process of averaging, will tend toward the complete cancelling of one another. Then the real error of any one of these independent results would be approximately given by its deviation from the average or mean result. It is, of course, very unusual to be able to command such series of measurements; but on that account it remains even more necessary to be ordinarily able to obtain an *estimate* of accuracy, and the method for doing so must therefore be developed.

For clearness, let us agree upon the following use of terms, and review some points concerning the nature of measurement and errors.

Let the term ***precision*** be used only with reference to the accordance of the individual results of a series; *i. e.*, their agreement among themselves or with the value adopted to represent the series, but without any reference to the possible divergence of the result from the truth.

Let the term ***accuracy*** be confined in its use to the latter case; *i. e.*, where it is desired to hold under consideration the observed result as a

representative of the *true* result, or as differing from it. The *accuracy* of a result would then be under consideration when its *errors* were referred to (*i. e.*, observed minus true values); but in dealing with *deviations* of single results from the mean or representative result or from each other, only the *precision* would be under consideration.

Direct Measurements are those made in such a way, or by aid of such instruments, as to obtain directly the quantity sought; *e. g.*, measurements of distance by a scale, of weights with a balance, of resistance with a bridge, etc. The direct readings may or may not require corrections.

Indirect Measurements are those where the desired result is not given directly by the measurement, but as the result of some combination of several observed values of different quantities,— in other words, where the result is a function of the observed quantities; *e. g.*, measurement of a distance by geodetic triangulation, of current by a tangent galvanometer or electro-dynamometer, of battery electro-motive force or resistance by two deflections on a tangent galvanometer, etc.

Quantities subjected to measurement may be either ***independent*** or ***conditioned.*** That is, two or any number of quantities may be wholly independent, so that the magnitude of one is in no way predetermined by any relations to the others; or they may be conditioned so that, for instance, the magnitude of two out of three being given, that of the third is thereby predetermined. Thus the current flowing through a given resistance might be anything whatever, according to the potential used, so that measurements of current and of resistance at any instant would be independent. But if the potential difference at the ends of the coil were simultaneously measured, then the three, current, resistance, and potential, would be conditioned by Ohm's law. The numerical values obtained in the measurement of conditioned quantities contain, of course, errors not controlled by these conditions, so that the values fail to fulfil the conditions, and require adjustment.

Every measurement, of whatever sort, is subject to errors. In coarse work it is easy to bring these within the limits allowable; in fine work, difficult or impossible. The sources of error may be regarded as of two classes, determinate and indeterminate.

Determinate Sources of Error. In any given investigation, a study of the instruments, methods, and conditions of observation employed in the light of all accessible knowledge in regard to these will reveal many possible sources of error, whose effects may be determined with greater or less closeness. Corrections may then be applied therefor, or the instruments, processes, and conditions so modified as to eliminate them. Such

sources of error would exist in the rate of a clock, in the graduation of a scale or circle, in the weights or beam of a balance, in the shape or adjustment of the coils of a galvanometer, etc. The study of these determinate errors is the most laborious part of an investigation, and is absolutely essential to even coarse work. Its neglect is repeatedly apparent in contradictory, and often useless results of elaborate work. But at the best, the correction and elimination of errors can be seldom more than approximate, and must leave behind residual errors, of greater or less magnitude, which will belong to the class of indeterminate errors; yet the attempt should always be made to render these residuals so small as at least not to exceed the average of the variable indeterminate errors. A general consideration of the nature of corrections, and of the closeness with which they should be determined, will be given later.

Indeterminate Sources of Error. Apart from the recognizable and determinate sources of error, there is a large number of unrecognizable, or at least indeterminate sources. To the nature of these, careful study may fail to give any clue. We only know that between successive measurements by the same process and between the results obtained by processes entirely or partly independent, discrepancies are always found. This demonstrates the perpetual existence of these indeterminate disturbing sources. They are thus indeterminate not in magnitude and sign only, but often in nature also. Some, however, are of known origin, but are not determinable on account of their small magnitude or of our inability to sufficiently control or measure their effects.

It is obvious that these indeterminate errors, in some degree, counterbalance each other; but there is always liability in any particular case to a preponderance of a single or resultant disturbing effect, which no process of averaging or combining the separate results can eliminate. Thus there will in general be a residual indeterminate error, which may be, and often is, surprisingly large. Such an error being naturally of a nearly constant amount (often called a "constant error"), would *not* be rendered apparent by the deviations among the single results. Hence the *precision* measure (p. 128), *i. e.*, the measure of concordance of the results, would give no intimation of its existence, and is, therefore, never a true *accuracy* measure. The only hope of a sufficient elimination of indeterminate errors lies in the plan already suggested — of combining *many* results obtained by *many distinct* methods and observers.

Corrections. The process of determining the amount of a correction is in itself, or involves, a subsidiary investigation. It may consist of such an operation as comparing a scale with a standard, of determining the rate of a clock, of computing the effect due to the size and form of galvanom-

eter coils, or to the short length of the mercury column in the exposed stem of a thermometer, etc. Few corrections can be found with absolute accuracy; in most cases the correction is determined by some physical process which will yield results of a desired accuracy, but will itself be subject to sources of error, which, in their turn, must be properly eliminated, and so on. Thus the study of the correction must be carried out under such conditions as to insure that its determinate errors have been so far removed that their residuals, in their turn, may properly be classed among the indeterminate errors. As to how small the precision measure of the correction should be rendered when all recognizable sources of error have been removed, it may here be said that this will be determined by two things, — the precision desired in the main result, and the labor involved in ascertaining the correction.

From this, it is obvious that if it were necessary in an investigation to work out from the beginning every detail of the research, establishing all standards, ascertaining all corrections, developing every process employed, the labor would be enormous — as, indeed, it often is. But fortunately the progress of experimental science has provided instruments, processes, methods, and results of known accuracy, which may be appropriated in any desired manner in more complex investigations.

Deviations vs. Errors. What is known of the laws *of error* is purely the result of inference. True values are never absolutely known. Let a_1, a_2, etc., be single observations of a quantity of which the true value is R. Their errors would be $E_1 = a_1 - R$, $E_2 = a_2 - R$, etc., but these cannot be known as R in unknown. Let A be the value chosen as the best representative (*e. g.* arithmetical mean) of the series. Then we know $d_1 = a_1 - A$, $d_2 = a_2 - A$, etc., which may be called the ***deviations*** of the single results from the best value, however that may be arrived at. The laws of these deviations have been studied for many different series of actual measurements. They are not the same for all cases. The following is, however, the most general case, and that from which the law of "accidental errors" is inferred.

Suppose an *indefinitely large number of equally careful* observations of a quantity made in a great variety of ways, and under widely different conditions: prolonged experience shows that the deviations of the single observations from the mean, in such a series, are distributed according to a definite law, which is approximated to more closely as the number of observations is greater, and which may be represented by an exponential equation. Of this distribution, it may be said that + and — deviations of any given magnitude are equally frequent; that small deviations are more frequent than large ones; that very large ones occur very seldom.

In such a series we should readily admit that sensibly all sources of *error* must have occurred in all possible ways. If so, we should suppose from the laws of chance, considered purely from a mathematical standpoint, that probably + and — errors of any given magnitude would be equally frequent, that small errors would be more frequent than large ones, and that very large errors would occur very seldom. But these statements are identical with those just found actually to hold for the deviations from the mean in the same series. Thus, *for such a series, the deviations may be assumed as sensibly coincident with the errors, and the mean with the true result,* which justifies the statement made at the outset (p. 122).

Best Representative Value. But the indefinitely large series is, of course, unattainable in practice, when the number of observations must generally be small. For smaller series the distribution of the *individual results about the mean* may be (usually is) approximately the same as for large series,—more closely as the number is larger, but as already stated in other words, we have no assurance that the deviations are accidental *errors,* as the mean may be, and very likely is, affected by some considerable resultant or constant error. The process of averaging *tends* to the elimination of the variable parts of the errors; and it may therefore be shown by the theory of probabilities, or experimentally, that even for small series the arithmetical mean is the best representative value, *provided that determinate errors have, as far as discoverable, been removed.* That is, the mean is the most probable value,—the one which in the long run will give the closest approach to the true result. Since the mean and true result will generally not coincide, there will be residual errors. These will themselves, in the long run, be distributed according to the general law of accidental errors, which is another indication that the mean is the best value to use. For that would naturally be the best value which, in the long run, would so distribute the residuals that their algebraic sum would be zero.

It thus appears that in order to get at a measure or estimate of the accuracy of the result of a completed investigation, we must study, first, all discoverable sources of error, in order to ascertain how far those which are determinate have been corrected or eliminated. Second, we must compute a measure of the disturbance due to the *variable portion* of the remaining indeterminate sources of error, so far, at least, as the measure of precision (p. 128) will give this. But it must always be remembered that beyond all this there *may,* and in general *must,* remain undetected "constant" errors. The discussion of the determinate errors is by far the most important of the two points above referred to. The discovery

of an undetected source of error in a result, furnishes, of course, a means of estimating its minimum error, or possibly of correcting it. If several measurements of the same quantity by different processes and observers exist, then the study of each, and the comparison and combination of all, will furnish the best available means of estimating the accuracy of the knowledge regarding that quantity.

The ideal procedure in a quantitative investigation would be this: Make a preliminary survey of the ground to be covered. Then make a detailed survey, and map out the general method and theory of the work. This includes studies of the apparatus to be used; of the procedure in observing; of the method of computing or "reducing" the results (the importance of having this thoroughly worked out in advance is very great); of the determination of the conditions under which the observations are to be made; of the distribution of accuracy necessary in the component measurements; of the nature of the instrumental and other corrections; of the best means of obtaining check results, etc. Next would come the preliminary observations for evaluating, *i. e.*, determining the value of, the corrections, etc. Then come the measurements proper of the investigation. Finally, there is the reduction, discussion, and presentation of these results. These last include not merely a computation and tabulation of the numerical results, but a complete discussion of their significance, a critical review of the work, and a thorough presentation of the whole, with data, when practicable, sufficient for a re-discussion of the work by others.

Three points may be mentioned in which quantitative investigations often fail to satisfy reasonable demands. First, the theory of the method is insufficiently studied in advance of the measurements, so that the adjustment of accuracy in the various component parts of the work is faulty, and the determinate errors are incompletely eliminated. Second, the results are presented in a very imperfectly digested form, so that but a very small part of their proper outcome and significance is obtained. Third, the data and results are so meagerly given that it is impossible to form any independent judgment of the real merit of the work.

It is not superflous to remark that the sole object of scientific investigation is the ascertainment of truth; and scientific work to be deserving of such classification must be conducted throughout in this spirit. The scientific observer must not only be above suspicion, but must be continually on his guard against bias and prejudice. Also, as the object of most investigation is to add to the known truth, it goes without saying that the worker should never fail to equip himself beforehand with a knowledge of all that has been previously done in the line of his pro-

posed work. Independent corroboration of work is of very great value; but it is likely to be of greater value when done in view of, than when in ignorance of, previously existing work.

The topics and formulæ necessary for the discussion of the relative distribution of precision in the component measurements of a complex quantity, of the best conditions for making certain measurements, and other analogous matter, will be now taken up, and illustrative examples will be given.

Measure of Precision. Bearing in mind the denotation already fixed upon for the terms accuracy and precision, it will be understood that the precision measure can never be anything more than an index or measure of the accordance of the single observations in a series, or of the separate results (means or single observations) composing a group. For most physical uses the simplest and most direct measure of precision should be preferred, and this is the *average deviation.* In addition to simplicity of computation, this has the merit that its name states exactly what it is, and all that it is, and no more. It is free from the liability to mislead which embarrasses the use of such terms as "probable error," "mean error," etc., these being stumbling-blocks to beginners, and a frequent cause of misapprehension even to those who know the true meaning of the terms.

The ***average deviation*** of the single observations from the mean will be indicated by *ad.* This will denote the average *magnitude* of the deviations, and it may be computed by the formula

$$ad = \frac{\Sigma d}{n} \qquad \text{I.}$$

where, therefore, Σd is the arithmetical sum of the deviations without regard to algebraic sign. Since a mean result is more precise than a single observation in the proportion of the square root of the number of the observations from which it is deduced, we should have, as a corresponding measure of the precision of the mean,

$$AD = \frac{a\,d}{\sqrt{n}} \text{ or } = \frac{\Sigma d}{n\sqrt{n}} \qquad \text{II.}$$

which would be called the Average Deviation of the mean.

Instead of *ad* and *AD* are often employed the "probable errors," which may be computed as

$$pe = 0.674\sqrt{\frac{\Sigma d^2}{n-1}}, \text{ or approximately } 0.85\ ad, \text{ or } 0.9\ ad,$$

and $PE = \frac{p\,e}{\sqrt{n}}$. The "error of mean square" $= \sqrt{\frac{\Sigma d^2}{n-1}}$ has been employed to a considerable extent in Germany.

The formula for the average deviation for indefinitely large series of observations following *any* law of distribution may be deduced thus:—

Δ = a deviation.

Let $\varphi(\Delta)$ = the law of deviation. The general law of deviations already referred to (p. 125) is not the only one; another is given further on in these notes. $\varphi(\Delta)$ is such a function that

$\varphi(\Delta)\cdot d\Delta$ = the probability that any single observation will have a deviation lying between Δ and $\Delta + d\Delta$; or $\varphi(\Delta).d\Delta$ = the "frequency" of occurrence of observations with deviations within that limit.

n = total number of observations in the series.

$\therefore\ n\cdot\varphi(\Delta)\cdot d\Delta$ = *number* of observations with deviations between Δ and $\Delta + d\Delta$.

$n\Delta\cdot\varphi(\Delta)\cdot d\Delta$ = sum of all the deviations between Δ and $\Delta + d\Delta$.

$\int_o^\infty n\Delta\cdot\varphi(\Delta)\cdot d\Delta$ = sum of all the deviations between $\Delta = \infty$ and $\Delta = 0$, *i. e.*, of all the positive deviations.

$2\int_o^\infty n\Delta.\varphi(\Delta)\cdot d\Delta$ = sum of magnitudes of the n deviations.

$\frac{2}{n}\int_o^\infty n\Delta\cdot\varphi(\Delta)\cdot d\Delta = ad$ = average of all the deviations between $+\infty$ and $-\infty$, *i.e.*, average magnitude.

$$\therefore\ ad = 2\int_o^\infty \Delta\cdot\varphi(\Delta)\cdot d\Delta. \qquad \text{III.}$$

If the deviations range only between the limits $+a$ to $-a$

$$ad = 2\int_o^a \Delta\cdot\varphi(\Delta)\cdot d\Delta. \qquad \text{IV.}$$

In any individual case it is therefore necessary to know a and $\varphi(\Delta)$ in order to have a determinate expression for ad.

Special Law of Deviations. 1st. There frequently occurs in practice the case where all values of Δ between $\Delta = +a$ and $\Delta = -a$ are equally likely to be obtained; *i.e.*, are equally frequent. Then $\varphi(\Delta)$ = a constant. Bearing in mind that the sum of all possibilities must be always unity, and also the above definition of $\varphi(\Delta)$, we have $\int_{-a}^{+a} \varphi(\Delta)\cdot d\Delta = 1$. Remembering that $\varphi(\Delta)$ is constant, and integrating gives $\varphi(\Delta)\int_{-a}^{+a} d\Delta = 1 = 2a\varphi(\Delta)$;

$$\therefore\ \varphi(\Delta)=\frac{1}{2a},\ \text{and}\ ad=2\int_o^a \Delta\cdot\varphi(\Delta)\cdot d\Delta=\frac{a}{2} \qquad \text{V.}$$

2d. Two independent and simultaneous sources of deviation, each following the law of 1st case. $ad=\frac{2}{3}a$. VI. Other cases may arise. [Both the foregoing and this demonstration are from Wright's "Adjustment of Errors of Observation."]

Principle of Least Squares. From the general law of distribution of deviations, or accidental errors, two deductions of importance may be made, viz. : —

1. That in combining a large number of equally reliable observations, whether direct or indirect, and whether of one or several independent variables, the most probable, or "best" result will be that which makes the sum of the squares of the deviations from that result a minimum. This is called the principle of least squares, and finds wide application. The use of the arithmetical mean may be shown to be in accordance with this principle.

2. That of means deduced respectively from n_1, n_2, etc., measurements, all equally reliable, the relative precision is in proportion to the square roots of the number of measurements; that is, of $\sqrt{n_1} : \sqrt{n_2}$: etc. Thus the mean of n equally reliable results is more precise than a single observation in the proportion of $\sqrt{n}$: 1 ; and hence $AD=\frac{ad}{\sqrt{n}}$ may be used as the precision measure of the mean, as stated at p. 128.

If the measurements are not all equally reliable, they may be made so by the application of their respective weights. It is shown from the law of deviations, that the weights of observations are inversely proportional to their AD^2, PE^2, etc. Let p denote the weight, then

$$p_1 : p_2 : p_3 = \frac{1}{AD_1^2} : \frac{1}{AD_2^2} : \frac{1}{AD_3^2} = \frac{1}{PE_1^2} : \frac{1}{PE_2^2} : \frac{1}{PE_3^2} = \text{etc.} \qquad \text{VII.}$$

In all the formulæ to be given, it will be assumed that the means and combinations are always made with due regard to weights.

Combination of Changes in Component Measurements. Suppose an indirectly determined quantity, M, to be related to the several *independent*, observed quantities, M_1, M_2, ... M_n, from which it is computed by the general expression

$$M=f(M_1, M_2, \ldots M_n) \qquad \text{VIII.}$$

Separate Effects. 1st. What effect would a change δ_1 or δ_2, etc., in any one of the variables M_1, M_2, etc., taken separately, have on M? It is obvious that this may be found from the differential of M with respect

to M_1 or M_2, etc. Thus, for example, denoting for brevity, $f(M_1, M_2, \ldots M_n)$ by $f(\ )$,

$$\frac{dM}{dM_1} = \frac{d}{dM_1}\left\{f(M_1, M_2, \ldots M_n)\right\}; \text{ or } \frac{dM}{dM_1} = \frac{df(\ )}{dM_1}; \text{ etc.} \qquad \text{IX.}$$

For finite changes, δ or Δ, we have the approximation $\frac{dM}{dM_1} = \frac{\Delta' M}{\delta_1}$, etc., whence approximately,

$$\frac{\Delta' M}{\delta_1} = \frac{df(\ )}{dM_1}, \quad \Delta' M = \frac{df(\ )}{dM_1}.\delta_1, \quad \Delta'' M = \frac{df(\ )}{dM_2}.\delta_2, \text{ etc.} \qquad \text{X.}$$

2d. What changes δ_1, δ_2, etc , in the components M_1, M_2, etc., would, if taken separately, each produce an assigned change $\Delta' M$, $\Delta'' M$, etc. in M? Obviously, this is merely the converse of the 1st case, whence

$$\frac{\Delta' M}{\delta_1} = \frac{df(\ )}{dM_1}, \text{ etc.} \quad \therefore \ \delta_1 = \Delta' M \Big/ \frac{df(\ )}{dM_1}, \ \delta_2 = \Delta'' M \Big/ \frac{df(\ )}{dM_2}, \text{ etc.} \qquad \text{XI.}$$

Combined or Collective Effects. The principle of least squares shows that for the general case where $M = f(M_1, M_2, \ldots M_n)$, M_1, M_2, etc. being *independent* variables, the combined effect of the separate changes $\Delta' M$, $\Delta'' M$, etc. will be given *best; i. e.*, given with greatest probability in the long run, by the expression

$$(\Delta M)^2 = (\Delta' M)^2 + (\Delta'' M)^2 + \ldots + (\Delta'' M_n)^2 \qquad \text{XII.}$$

which accords with the statement made previously in the paragraphs of p. 130. Hence, by substituting the above equivalents of $\Delta' M$, $\Delta'' M$, etc., and, for simplicity, writing Δ for ΔM, we have the following formulæ: Those for the special values of $f(M_1, M_2, \ldots M_n)$ are deduced directly from the general case by inserting the values of $f(\ )$, and performing the differentiation. b = a known constant.

$M = f(M_1, M_2, \ldots M_n)$

$$\Delta^2 = \left(\frac{df(\ )}{dM_1}.\delta_1\right)^2 + \left(\frac{df(\ )}{dM_2}.\delta_2\right)^2 + \ldots + \left(\frac{df(\ )}{dM_n}.\delta_n\right)^2 \qquad \text{XIII.}$$

$M = M_1 \pm M_2 \pm \ldots \pm M_n$

$$\Delta^2 = \delta_1^2 + \delta_2^2 + \ldots + \delta_n^2 \qquad \text{XIV.}$$

$M = b_1 M_1 \pm b_2 M_2 \pm \ldots \pm b_n M_n$

$$\Delta^2 = (b_1\delta_1)^2 + (b_2\delta_2)^2 + \ldots + (b_n\delta_n)^2 \qquad \text{XV.}$$

$M = b \cdot M_1 \cdot M_2 \cdots M_n$

$$\left(\frac{\Delta}{M}\right)^2 = \left(\frac{\delta_1}{M_1}\right)^2 + \left(\frac{\delta_2}{M_2}\right)^2 + \ldots + \left(\frac{\delta_n}{M_n}\right)^2 \qquad \text{XVI.}$$

$$M=\frac{b_1 \cdot M_1 \cdots}{b_2 \cdot M_2 \cdots}$$

$$\left(\frac{\Delta}{M}\right)^2 = \text{same as preceding.} \qquad \text{XVII.}$$

$$M=b \cdot M_1^{\pm n} \quad \Delta^2=\left(\frac{df}{dM_1}\cdot\delta_1\right)^2=\left(\pm nbM_1^{\pm n-1}\delta_1\right)^2$$

$$\therefore \left(\frac{\Delta}{M}\right)^2=\left(\frac{\pm n\delta_1}{M_1}\right)^2 \quad \therefore \frac{\Delta}{M}=\pm\frac{n\delta_1}{M_1} \qquad \text{XVIII.}$$

$$M=\varphi(M_1)\cdot\rho(M_2)\cdots\sigma(M_n)$$

$$\left(\frac{\Delta}{M}\right)^2=\left(\frac{d\varphi(M_1)}{dM_1}\cdot\frac{\delta_1}{\varphi(M_1)}\right)^2+\left(\frac{d\rho(M_2)}{dM_2}\cdot\frac{\delta_2}{\rho(M_2)}\right)^2+\cdots$$

$$+\left(\frac{d\sigma(M_n)}{dM_n}\cdot\frac{\delta_n}{\sigma(M_n)}\right)^2 \text{XIX.}$$

$$\left(\frac{\Delta}{M}\right)^2=\left(\frac{\delta\varphi(M_1)}{\varphi(M_1)}\right)^2+\left(\frac{\delta\rho(M_2)}{\rho(M_2)}\right)^2+\ldots+\left(\frac{\delta\rho(M_n)}{\rho(M_n)}\right)^2 \quad \text{XX.}$$

Of the special cases, the two most common may readily be put into words. When the f is the sum or difference, the square of the resultant change *in units* = the sum of the squares of the component changes *in units*. When f is a product, or quotient, the square of the resultant *fractional* change = the sum of the squares of the component *fractional* changes. The last special case calls for the following further notice.

Separation into Factors. We have often to deal with an expression which is a somewhat complicated function of several independent variables, so that its differentiation is cumbrous, but which may readily be separated into factors, each of which is a function of *but one* independent variable. Factoring thus would give $M=\varphi(M_1)\,.\,\rho(M_2)\ldots\sigma(M_n)$ as in XIX. Then substituting in XIII, and carrying the differentiation as far as we can, *i. e.*, to the indicated differential $\frac{d\varphi(M_1)}{dM_1}$, etc., we deduce the form of XIX. But if for differentials we substitute finite changes, we may write for $\frac{d\varphi(M_1)}{dM_1}$, $\frac{\delta\varphi(M_1)}{\delta M_1}$; etc. Then

$$\left(\frac{\Delta}{M}\right)^2=\left(\frac{\delta\varphi(M_1)}{\delta M_1}\cdot\frac{\delta_1}{\varphi(M_1)}\right)^2+\left(\frac{\delta\rho(M_2)}{\delta M_2}\cdot\frac{\delta_2}{\rho(M_2)}\right)^2+\ldots+\left(\frac{\delta\sigma(M_n)}{\delta M_n}\cdot\frac{\delta_n}{\sigma(M_n)}\right)^2$$

which becomes identical with XX when we cancel δ_1 and δM_1, which are merely two expressions for the same quantity, provided that we remember that $\delta\varphi(M_1)$ etc., are the changes due to δ_1, etc., in the variables M_1, etc. This process is often a convenience, and illustrations of its use will be given later. In words, expression XX becomes, the square of the resultant

fractional change = the sum of the squares of the fractional changes *in the factors.*

Applications. For present purposes these formulæ are to be used in computing the effects upon the final result due to deviations or errors in the component measurements. The values to be used for δ will thus be precision measures or errors, as the case may be. The discussions will be intended to apply especially to cases where the number n of the variables is small, as is usual in physical work. In order to secure for the formulæ the most favorable conditions, the value of n being in general small, no determinate error should be allowed to enter into the precision measures. If the effect of such is under consideration, it must be separately studied by independent differentiation. Such cases often occur in considering the effect of omitting known corrections, or of dropping significant figures from known numerical constants, etc.

It has been shown already that when all determinate sources of error have been as far as possible eliminated, there remain only precision measures to be considered. Hence in the equations for collective effects, we have properly to deal only with precision measures as δ's, and thus the resulting Δ is a precision measure of the same kind as the δ's. In any single equation, of course, all the δ's must be of the same kind; *e. g.*, all must be *ad*'s and *AD*'s, or all *pe*'s and *PE*'s, etc. A $\delta_1 = ad$ may obviously be combined with a $\delta_2 = AD$, if M_1 is a single measurement and M_2 a mean, but an *ad* cannot be combined with a *pe*. The resulting Δ will necessarily be an *ad* if any one or more of the δ's is an *ad*; but if all δ's are AD's, then the Δ may of course be regarded as a AD. This classification of the Δ is, however, unimportant. The point is, that Δ is the *precision* measure of the result M. If any component, *e. g.* M_1, were obtained as a direct uncorrected measurement, then its δ would be simply the average deviation of the measurement. But if this component M_2 is in itself a complex or indirect quantity, as is usual, its δ must include the measures of all sources of deviation, viz., from observations, corrections, component measurements, etc.

The formulæ are applicable to two somewhat distinct cases.

1st. Given values of $\delta_1, \delta_2, \ldots, \delta_n$, to compute their effect, either separately or together, on M.

2d. Prescribed a definite limit of precision or accuracy in the final result; to compute beforehand what grade of *precision* or *accuracy* (to be expressed by δ_1, δ_2, etc.) is necessary in each component in order that the final result shall not be affected beyond the assigned limit.

In the second of these applications it is, of course, essential to know beforehand roughly approximate values of the various quantities, M_1, M_2,

etc., but very roughly measured, and even estimated or computed values, are usually sufficient. Thus these applications can generally be made in advance of the construction of apparatus for an investigation, and may serve to show whether a proposed investigation, or instrument, or process, probably can or cannot be made to yield a desired result, or to show what the proportion of parts and the arrangement of apparatus should be. They enable us also to deduce rules for the use of places of significant figures.

Distribution of Precision amongst Components. In making the second application, it is necessary to determine beforehand upon some relative distribution of precision amongst the components. The simplest would be that which would assign to the δ's such values that *each* should produce an *equal effect* on the final result. But this would not be invariably the best distribution, since it might conceivably demand an amount of labor in making one or more of the measurements which would be widely disproportionate to that involved in making the others. It cannot, however, be much departed from, and should usually be the basis of computation. Yet it is always of advantage to make any δ smaller than this requirement would indicate, if this can be done with little more than an equal share of labor; provided, however, that it be understood that there is but slight advantage in reducing any δ much below one tenth of its value for equal effects. The general solution for "equal effects" is given below.

Formulæ for Equal Effects. The formula for the "combination of errors" for the general case $M = f(M_1, M_2 \ldots M_n)$ has been given at p. 131, as M_1, etc., being independent variables.

$$\Delta^2 = \left(\frac{df()}{dM_1}.\delta_1\right)^2 + \left(\frac{df()}{dM_2}.\delta_2\right)^2 + \ldots + \left(\frac{df()}{dM_n}.\delta_n\right)^2 \qquad \text{XIII.}$$

Now, in order that each δ should have an equal effect on Δ, *i. e.*, that each source of deviation should have an equal effect with each of the others on the final result, the terms of the second member must be all equal. Thence,

$$\frac{df()}{dM_1}.\delta_1 = \frac{df()}{dM_2}.\delta_2 = \ldots = \frac{df()}{dM_n}.\delta_n = \frac{\Delta}{\sqrt{n}} \qquad \text{XXI.}$$

Hence,

$$\delta_1 = \frac{\Delta}{\sqrt{n}} \Big/ \frac{df()}{dM_1};\ \delta_2 = \frac{\Delta}{\sqrt{n}} \Big/ \frac{df()}{dM_2};\ \ldots;\ \delta_n = \frac{\Delta}{\sqrt{n}} \Big/ \frac{df()}{dM_n} \qquad \text{XXII.}$$

Ordinarily, the assigned limit for error in M is given as a fraction of M;

i. e., as $\frac{\Delta}{M}$, or as a percentage, which would be 100 $\frac{\Delta}{M}$. Of course the corresponding numerical value of Δ may be at once computed by multiplying by M, for $\Delta = \frac{\Delta}{M}.M$.

Applying the condition of equal effects to some special cases, we have the following :—

For $M = M_1 \pm M_2 \pm \ldots \pm M_n$, $\quad \Delta^2 = \delta_1^2 + \delta_2^2 + \ldots + \delta_n^2$ XIV.

Hence for equal effects of δ_1, δ_2, etc.,

$$\delta_1^2 = \delta_2^2 = \ldots = \delta_n^2 = \frac{\Delta^2}{n} \quad \therefore \delta_1 = \delta_2 = \ldots = \delta_n = \frac{\Delta}{\sqrt{n}} \qquad \text{XXIII.}$$

For $M = b.M_1.M_2 \cdots M_n$, $\left(\frac{\Delta}{M}\right)^2 = \left(\frac{\delta_1}{M_1}\right)^2 + \left(\frac{\delta_2}{M_2}\right)^2 + \ldots + \left(\frac{\delta_n}{M_n}\right)^2$ XVI.

Hence, for equal effects,

$$\frac{\delta_1}{M_1} = \frac{\delta_2}{M_2} = \ldots = \frac{\delta_n}{M_n} = \frac{1}{\sqrt{n}} \cdot \frac{\Delta}{M} \qquad \text{XXIV.}$$

For $M = \varphi(M_1).\rho(M_2) \cdots \sigma(M_n)$,

$$\left(\frac{\Delta}{M}\right)^2 = \left(\frac{\delta\varphi(M_1)}{\varphi(M_1)}\right)^2 + \left(\frac{\delta\rho(M_2)}{\rho(M_2)}\right)^2 + \ldots + \left(\frac{\sigma(\delta M_n)}{\sigma(M_n)}\right)^2 \qquad \text{XX.}$$

Hence, for equal effects,

$$\frac{\delta\varphi(M_1)}{\varphi(M_1)} = \frac{\delta\rho(M_2)}{\rho(M_2)} = \ldots = \frac{\delta\sigma(M_n)}{\sigma(M_n)} = \frac{1}{\sqrt{n}} \cdot \frac{\Delta}{M} \qquad \text{XXV.}$$

i. e., in this "separation into factors" the ***fractional change*** in every *factor* is the same, and is equal to $1/\sqrt{n}$ of the resultant or assigned fractional change in M. In general, however, we wish to find the changes not in the factors, but in the observed variables M_1, $M_2 \ldots M_n$. But having deduced the change in any factor, we may, of course, deduce the corresponding change in the variable contained in that factor by direct differentiation. For the change $\delta\varphi(M_1)$, produced by the change $\delta M_1 (= \delta_1)$, is given by the differential of $\varphi(M_1)$ with respect to M_1. That is,

$$\frac{\delta\varphi(M_1)}{\delta_1} = \frac{d\varphi(M_1)}{dM_1}$$

whence,

$$\delta_1 = \delta\varphi(M_1) \Big/ \frac{d\varphi(M_1)}{dM_1} \qquad \text{XXVI.}$$

similarly,

$$\delta_2 = \delta\rho(M_2) \Big/ \frac{d\rho(M_2)}{dM_2}; \text{ etc.}$$

The numerical values of $\delta\varphi(M_1)$, etc., for substitution in these are obtained, of course, from the fractional changes in the factors by multiplying these fractions by the numerical values of $\varphi(M_1)$, etc. That is,

$$\delta\varphi(M_1) = \frac{1}{\sqrt{n}} \cdot \frac{\Delta}{M} \cdot \varphi(M_1), \quad \delta\rho(M_2) = \frac{1}{\sqrt{n}} \cdot \frac{\Delta}{M} \cdot \rho(M_2), \text{ etc.} \qquad \text{XXVII.}$$

Or we may combine these two steps algebraically, and get

$$\delta_1 = \frac{1}{\sqrt{n}} \cdot \frac{\Delta}{M} \cdot \varphi(M_1) \Big/ \frac{d\varphi(M_1)}{dM_1} \quad . \quad . \quad . \qquad \text{XXVIII.}$$

$$\delta_2 = \frac{1}{\sqrt{n}} \cdot \frac{\Delta}{M} \cdot \rho(M_2) \Big/ \frac{d\rho(M_2)}{dM_2}, \text{ etc.,}$$

And these expressions may be seen, by inspection, to be identical with those which would have been obtained had XIX been subjected directly to the condition of equal effects.

In this method of using factors, it must be borne in mind that the results for δ_1, δ_2, etc., are the same as would be obtained by a direct application of the general process only when the factors are wholly independent of each other. Yet in some cases it is convenient to separate into factors which are not wholly independent. Such a process would, however, seldom be used unless the factors were but slightly dependent; for instance, where the variable of one factor entered into another factor merely in a small correction term, or in some other way, in which the resulting values of δ were not sensibly affected by the irregular separation

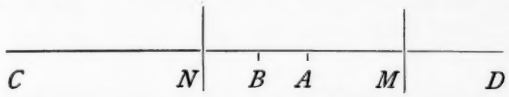
Simplification of Functions. Since the aim usually is to find, only roughly, approximate values of the δ's corresponding to an assigned Δ, much labor may be often saved by simplifying, as far as possible, the function $f(M_1, M_2, \ldots, M_n)$ before differentiation. The nature of the simplification would depend on the case in hand, and might consist in omitting small correction terms, the use of approximate formulæ, etc. Also, in the same investigation, it might be possible to use approximate or simplified function in studying some variables, and the complete function in studying others, and different simplifications for various parts of the work.

THE MEAN PLANE OF A GALVANOMETER COIL.

BY J. J. SKINNER, PH.D.

IT is commonly assumed that the mean plane of a galvanometer coil is a plane such that at equal distances on each side of it in the axis of the coil a suspended magnet needle will be equally deflected. We may show that if a coil is not symmetrically wound with reference to its geometrical mean plane there is, strictly speaking, no fixed plane which shall have the above property for varying distances of the needle, but also that in common galvanometer coils the assumption that there is a fixed mean plane is sufficiently exact.

Assume, at starting, two parallel and coaxial coils, M and N, the first having m turns of wire, and the second n turns; the sections of both coils being infinitely small, their mean radii being equal, and denoted by r, and the distance between the planes of the coils being denoted by $2b$.



Drawing a figure in which a line $C\ D$ shall represent the common axis of the coils, their centres being thereon at M and N, let A be the middle point between M and N; and let the assumed mean plane of the two coils cut the axis at such a point B, that if $B\ C$ and $B\ D$ are each equal to e, the force on a needle at C, due to a current flowing in series through the wire of the two coils, will be equal to the corresponding force at D. Let x denote the distance $A\ B$. We may assume that the current is equal to unity.

If the needle is at C, the force there due to both coils will be

$$\frac{2\pi r^2 m}{[(e+b+x)^2+r^2]^{\frac{3}{2}}} + \frac{2\pi r^2 n}{[(e-b+x)^2+r^2]^{\frac{3}{2}}} \quad . \quad . \quad . \quad (1)$$

and if the needle is at D, the force there due to both coils will be

$$\frac{2\pi r^2 m}{[(e-b-x)^2+r^2]^{\frac{3}{2}}} + \frac{2\pi r^2 n}{[(e+b-x)^2+r^2]^{\frac{3}{2}}} \quad . \quad . \quad . \quad (2)$$

Placing (1) and (2) equal to each other, we have

$$\frac{m}{[(e+b+x)^2+r^2]^{\frac{3}{2}}} + \frac{n}{[(e-b+x)^2+r^2]^{\frac{3}{2}}}$$

$$= \frac{m}{[(e-b-x)^2+r^2]^{\frac{3}{2}}} + \frac{n}{[(e+b-x)^2+r^2]^{\frac{3}{2}}} \quad . \quad . \quad . \quad (3)$$

The first term of this equation may be written

$$\frac{m}{[(e+b)^2+2(e+b)x+x^2+r^2]^{\frac{3}{2}}}$$

and if x is small compared to b, as it evidently will be if the difference between m and n is not too large, this expression may be written approximately

$$\frac{m}{[r^2+(e+b)^2]^{\frac{3}{2}}\left\{1+\frac{2(e+b)x}{r^2+(e+b)^2}\right\}^{\frac{3}{2}}}$$

which, by further approximate reduction, becomes

$$\frac{m\left\{1-\frac{3(e+b)x}{r^2+(e+b)^2}\right\}}{[r^2+(e+b)^2]^{\frac{3}{2}}} \quad . \quad . \quad . \quad (4)$$

By comparison, the other terms of equation (3) may be written in a form similar to (4), with proper changes of sign; so that equation (3) will become

$$\frac{m\left\{1-\frac{3(e+b)x}{r^2+(e+b)^2}\right\}}{[r^2+(e+b)^2]^{\frac{3}{2}}}+\frac{n\left\{1-\frac{3(e-b)x}{r^2+(e-b)^2}\right\}}{[r^2+(e-b)^2]^{\frac{3}{2}}}$$
$$=\frac{m\left\{1+\frac{3(e-b)x}{r^2+(e-b)^2}\right\}}{[r^2+(e-b)^2]^{\frac{3}{2}}}+\frac{n\left\{1+\frac{3(e+b)x}{r^2+(e+b)^2}\right\}}{[r^2+(e+b)^2]^{\frac{3}{2}}}$$

which again reduces to

$$\frac{3(m+n)(e+b)x}{[r^2+(e+b)^2]^{\frac{3}{2}}}+\frac{3(m+n)(e-b)x}{[r^2+(e-b)^2]^{\frac{3}{2}}}=\frac{n-m}{[r^2+(e-b)^2]^{\frac{3}{2}}}-\frac{n-m}{[r^2+(e+b)^2]^{\frac{3}{2}}} \quad (5)$$

from which the value of x is easily written as a complex function of e.

It is therefore clear that there is no fixed plane for the two coils under discussion such that equal forces shall be exerted on a needle at all equal distances on each side of the plane. Hence it follows that if a coil is made up of wire wound in many planes, a greater or less crowding together of the planes in the different parts, so as to give an unsymmetrical density along the axis, would lead to the result that there would, strictly speaking, be no mean plane of the coil; that is, there would be no plane such that at all equal distances on the two sides of the plane the forces on a needle in the axis should be equal.

We may find the value of x for the special case of formula (5) when e increases without limit, as follows: Dropping r^2, as in this case negligi-

ble in comparison with $(e+b)^2$ and $(e-b)^2$, and then finding the value of x from (5), we get

$$x=\frac{(n-m)[(e+b)^3-(e-b)^3](e^2-b^2)^2}{3(n+m)[(e+b)(e-b)^5+(e-b)(e+b)^5]} \quad . \quad . \quad . \quad (6)$$

the limit of which, as e increases to infinity, is

$$x=\frac{b(n-m)}{n+m} \quad . \quad . \quad . \quad (7)$$

To find the limiting value of x in the special case when e diminishes toward zero, we may write the first term of equation (3) in the approximate form

$$\frac{m}{[r^2+b^2+x^2+2(b+e)x+2be]^{\frac{3}{2}}} \quad . \quad . \quad . \quad (8)$$

or by another approximation

$$\frac{m\left(1-\frac{3(b+e)x+3be}{r^2+b^2+x^2}\right)}{(r^2+b^2+x^2)^{\frac{3}{2}}} \quad . \quad . \quad . \quad (9)$$

By putting all the terms of equation (3) into a form analogous with this, and then reducing, we get

$$x=\frac{b(n-m)}{n+m} \quad . \quad . \quad . \quad (10)$$

which is the same value that we found by making $x=\infty$. (Eq. 7.)

There is no value of e which shall make $x=0$ when m and n are not equal, and when b is not 0; since by putting $x=0$ in equation (5), we find $b=0$. Hence, as x, from equation (5), is always real, it will always, with given values of m and n, be of the same sign. In the figure, x was taken positive when measured to the left of A; and since from (7) and (10) x is positive when n is greater than m, we see that the mean plane for any value of e is always nearer to the coil having the larger number of turns of wire.

If $e=b$, equation (5) gives

$$x=\frac{(n-m)(r^2+4b^2)[(r^2+4b^2)^{\frac{3}{2}}-r^3]}{6(m+n)br^3} \quad . \quad . \quad . \quad (11)$$

which is evidently positive when n is greater than m; and when b is very small compared to r, this equation also reduces approximately to the same value as that of x in equations (7) and (10).

In fact, if b is very small compared to r, equation (7) gives an approximate value of x for *all* values of e. For, from the nature of the case, x

is always smaller than b, and the first term of equation (3) may be written without approximation in the form

$$\frac{m}{[r^2+e^2+b^2+x^2]^{\frac{3}{2}}\left\{1+\frac{2(eb+ex+bx)}{r^2+e^2+b^2+x^2}\right\}^{\frac{3}{2}}} \quad . \quad . \quad . \quad (12)$$

Now, supposing b (and consequently x) to be small compared to r, the fraction $\frac{2(eb+ex+bx)}{r^2+e^2+b^2+x^2}$ will be small for all values of e; so that we may write expression (12) in the approximate form

$$\frac{m\left(1-\frac{3(eb+ex+bx)}{r^2+e^2+b^2+x^2}\right)}{(r^2+e^2+b^2+x^2)^{\frac{3}{2}}} \quad . \quad . \quad . \quad (13)$$

Bringing all the other terms of equation (3) to this form by comparison, and then reducing, we find the same value for x as that given in equation (7).

Thus, although x is in general a function of e, yet with values of b that are small in comparison with r, the value of x will vary but very slightly from its expression in equation (7), whatever be the value of e.

Hence, although we might conclude from equation (5) that if a galvanometer coil has a small radius and a wide section, an extreme crowding of the wire in certain planes might result in a variable position of the "mean plane," depending on the distance of the test needle; on the other hand, the fact that when b is small compared to r the value of x is sensibly constant for all values of e, shows that with ordinary coils no liability of such a result need be feared.

EXPERIMENTS ILLUSTRATING THE DESCENT OF THE CHARGE IN AN IRON BLAST-FURNACE.

BY ROBERT H. RICHARDS AND RICHARD W. LODGE, OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY, BOSTON, MASS.

Read before the American Institute of Mining Engineers at Duluth Meeting, July, 1887.

A GREAT deal of speculation, as well as actual experiment, has been devoted to ascertaining the changes in the materials during their descent in an iron blast-furnace, (1) chemical, (2) mechanical, and (3) arrangement of the materials. With a view to throw some light upon the third question, we constructed a model and tried experiments which are here described.

Our model is a vertical section of a furnace, with the lines of the Edgar Thomson furnace D 1885, and for the use of the latter we are indebted to Mr. Gayley. The model is 40 inches high, and made to scale ($\frac{1}{2}$ inch equals 1 foot), and the horizontal section of the space in which the charge falls at any point in the descent is $1\frac{1}{2}$ in. $\times$ d, d being the diameter of the furnace at that place.

We are unable to say just where the idea of making this model came from; perhaps, partly from Prin. Manf. Iron and Steel, 1884, I. Lowthian Bell, p. 124; partly, also, from an indistinct recollection of some one's having reported to us that he had made such a model. The facts, however, are gone from memory now.

The back and sides of the model are of $\frac{7}{8}$-inch pine board; the front is $\frac{1}{4}$-inch plate-glass, screwed to the sides. The space between the glass front and the board back is $1\frac{1}{2}$ inch. Below is a little bin into which the charge falls, and from which it is shoveled back to the bell and hopper. Every shovelful withdrawn from below settles the contents of the stack sufficiently to receive a new charge above, and in this way a great variety of feeds may be speedily tried, and the arrangement due to them inspected through the plate glass.

The material used for the charge in these tests was composed of beach pebbles and clean quartz-mill sand 40-mesh, and the different sizes were mixed in the proportion indicated below, making four lots: —

	Lot I. Per cent.	Lot II. Per cent.	Lot III. Per cent.	Lot IV. Per cent.
Through $\frac{1}{2}$-inch sieve on $\frac{1}{4}$-inch sieve . . .	38.74	51.66	64.0	76.08
Through $\frac{1}{4}$-inch sieve on $\frac{1}{8}$-inch sieve . . .	2.56	1.81	2.0	2.43
Through $\frac{1}{8}$-inch sieve on $\frac{1}{16}$-inch sieve . . .	13.67	11.17	9.0	6.39
Through $\frac{1}{16}$-inch sieve on $\frac{1}{30}$-inch seive . .	4.56	3.32	2.3	1.40
Through $\frac{1}{30}$-inch sieve	40.46	32.02	22.70	13.69

Each lot of material, so mixed, was used for a series of tests, and in every case, as soon as the furnace had got into normal running, a photograph of the condition of the charge at that time was taken. These photographs are in series, with two variables; namely, first, the kind of bell or distributor, and secondly, the proportion of coarse and fine in the material used.

The Roman numerals, I, II, III, IV, are used to designate the lot of material referred to, and the letters, a, b, c, etc., are used to designate the bells or distributors which were used — a, large bell; b, small bell; c, no bell at all.

Figs. I, II, III, IV, with variable gravel in series, will be seen to start with a large proportion of fine I, and gradually become coarser till IV, is reached, where there is a large proportion of coarse.

Figs. a, b, c, with variable bell in series, will be seen to start with a (large bell), having three columns placed thus — a column of coarse material in the centre with fine at the two sides; then comes b (small bell), with five columns, having coarse in the centre and an annulus of fine representing two intermdiate columns and two columns of coarse again on the outside; finally, c (no bell), which is fairly well mixed, except that there is a trifling excess of coarse material on the margin.

Fig. I f, has been taken since the paper was written; it represents a furnace with a funnel delivering the whole of the gravel at the centre of the stock-line, and in it will be found two columns of coarse on the sides, and a column of fine in the centre.

Figs. d, and e, were only tried for one series, and they go to show that with a small bell, if the stock is not kept up to the proper stock-line, the arrangement approaches a, in e, nearly so, and in d, quite so; *i. e.*, a small bell poorly fed does the work of a large bell well fed, at the expense, however, of loss of height in the furnace. See II a, II d, II e.

The large bell is the normal Edgar Thomson bell; the small bell is 4 inches in diameter, and was chosen not from any known practical proportions, but simply to see what would be the effect of a small bell.

Series I, shows isochronous lines, which connect particles that have been in the furnace the same length of time. These curves were obtained by putting in a layer of charcoal and chalking its position after every withdrawal of charge from below.

In examining these photographs, we notice: —

1. That a bell which is large, with reference to its stock-line, always gives the charge in three columns; the two outer are fine, the one inner column is coarse. See I a, II a, III a, IV a.

2. That a bell which is small, with reference to the stock-line, and which is kept full fed, always yields the charge in five columns. The centre is coarse, then an annulus of fine, and then margins of coarse again. See I b, II b, III b, IV b.

3. That a furnace fed without a bell, and whose top is smaller than the stock-line, will give a fairly even mixture, the coarse material being slightly concentrated in the margin. See I c, II c, III c, IV c. And that a furnace fed with a central funnel gives three columns; namely, fine in the centre, and two coarse columns on the sides. See I f.

4. That if the stock-line be varied from high to low, with a small bell, the arrangement of particles resulting will vary from the five columns to three columns. See II b, II e, II d.

5. Columns of coarse material appear to settle more rapidly; columns of fine, less rapidly. See I a, I b, I c, I f. Here the coarse in the centre

of I a appears to descend fully twice as fast as the fine on the sides. In I b, the coarse being on the sides as well as in the centre, the charge settles about evenly until the point is reached where the friction and squeezing of the boshes comes in, when the central coarse column pushes ahead. In I c, the coarse is slightly concentrated in the margin, and hence the margin settles a little the fastest until the boshes are reached; and finally in I f, the sides are wholly coarse, the centre fine, and here we have the sides gaining on the centre a little faster even than in I c.

If we are rightly informed, there is a general opinion among iron men that a hand-fed furnace is better fed than any other, but that the quantities now used forbid hand feed. The evenness of the feed in I c, II c, III c, and IV c, would seem to bear out this opinion. Again, in order to drive a furnace for great production, it is necessary to have a large bell (that is to say, large in the sense used in this paper), and one with a proper proportion to the stock line. Now the greatest danger in a hard-driven furnace would presumably be that the lining would give out; observe how the work is balanced for fast driving and brick saving in I a, II a, III a, IV a: here the side columns are fine, the gases penetrate slowly, reduction is slow, combustion is slow, heat cannot attain its highest degree, and the bricks are saved. To balance all this and prevent the furnace from going to white iron and black slag, this part of the charge moves slowly down through the furnace, and thus has longer time to pass through its chemical and physical changes. On the other hand, the centre of the charge moves rapidly down, has rapid moving gases, combustion is rapid, reduction is rapid, heat attains a high degree, and hence the ore can safely pass through its chemical and physical changes, even though the time allowed it is much shorter. If this idea be correct, then hand feed can only be applied to slow running furnaces, for the coarse will be at the sides I c, II c, III c, IV c, and with hard driving and great production will fail to protect the brick linings.

We presume that the forming of coarse and fine columns will have a tendency to produce an "unmixing" of the charge, and while at times this might cause trouble, it again might give the furnace man a chance to put in his medicine where it was wanted; for instance, he could put more lime in the centre and less on the sides, or vice versa by having the limestone coarser in the first instance and finer in the second. The same would be true also of the coke. It will take a person of more experience in iron than either of us to tell whether scaffolds could be removed by feeding, for several successive charges, three quarters of the bell with normal ore charge, and the other one quarter immediately over the scaffold with all coarse charge, and so for several successive charges to lead the

high heat over to the side where the scaffold was located, and in that way to partly scour and partly burn the obstruction off.

Fig. I g, was obtained while the model was being designed. The discharge at the bottom was only 1½ inches wide at that time, and the figure is put in simply to show how a perfectly stationary scaffold can form in this model, fed with dry gravel and sand, if only a little obstruction is left for it to rest upon. All the space behind the chalk line is scaffold.

We presume that the remark will occur to the reader that "a forty-inch furnace cannot reproduce exactly the conditions of an eighty-foot furnace." The following exceptions may be taken to experiments of this character: —

1. A rectangular horizontal section gives an untrue representation of the two opposite sectors taken from a circular horizontal section. The error here is that central columns shown in our diagrams are a little too narrow, and the side columns are a little too wide.

2. Our material is all of the same specific gravity, while iron ore, limestone, and coke vary a good deal. This might make a difference in the arrangement, but probably not much, as the fragments take their places according to size, the largest rolling the furthest, the smallest lodging almost immediately.

3. ½ inch and ¼ inch pebbles may be mathematically right for a 40-inch furnace as compared with the usual size, fed to an 80-foot furnace, but cannot, of course, exactly reproduce the effects.

4. The chute of the material with an 11-foot bell may differ in velocity from that of a 5-inch bell, but in each case they simply deliver the material at the angle due to the bell, which is the same in both cases.

5. In a rectangular sectional model the friction of the charge on glass in front and wood behind, would cause a different condition from that in a circular furnace, tending to retard the rapid moving particles more than the slow, and in this way flattening the isochronous curves.

6. The loosening of the charge in the large furnace by the combustion of the coke cannot be represented in the model. If the coke was evenly mixed, this would affect the settling evenly; but if, as is more likely the case, the coke is somewhat concentrated in the center, then the center would be accelerated, this fact would cause the isochronous curves in I c, I f, to be too convex, and those in I a, I b, to be too obtuse in the model.

We presume that exception 1 makes a noticeable difference, narrowing the inner columns and widening the outer; and that 5 and 6 both cause a greater retarding of the fast moving particles as compared with the slow moving particles, in this way giving flatter isochronous curves than would be true in the large furnace; nevertheless, crude and defective as the experiments are, we hope they may throw some light upon the difficult question of furnace feeding.

FIG. I *a*.

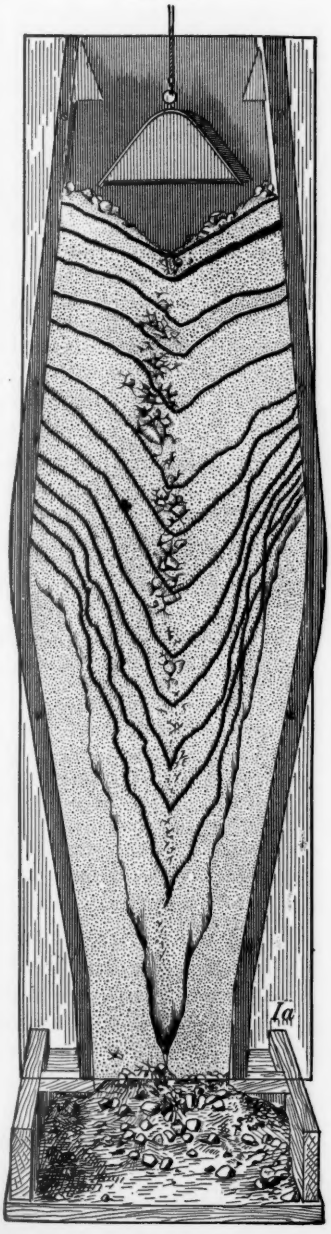


FIG. I *b*.

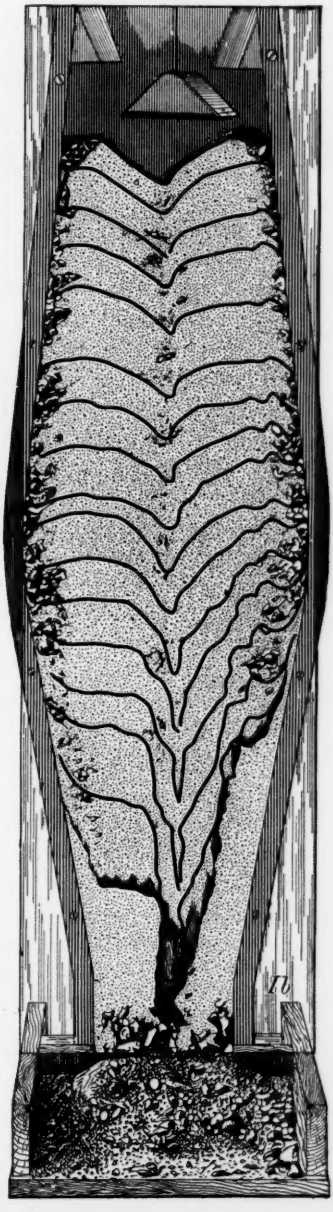


FIG. I*c*.

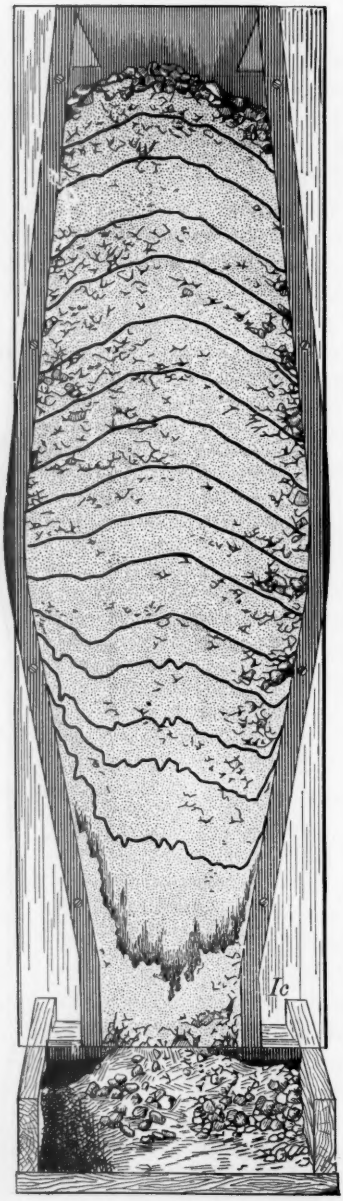


FIG. I*f*.

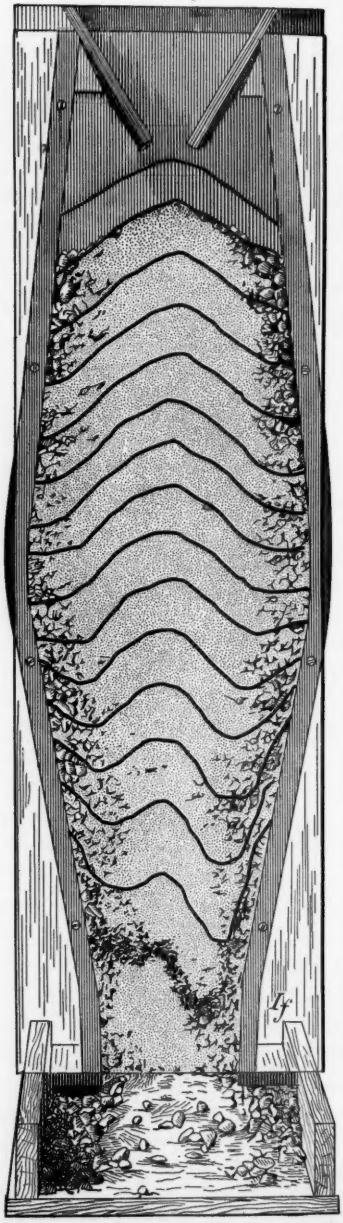


FIG. II *a*.

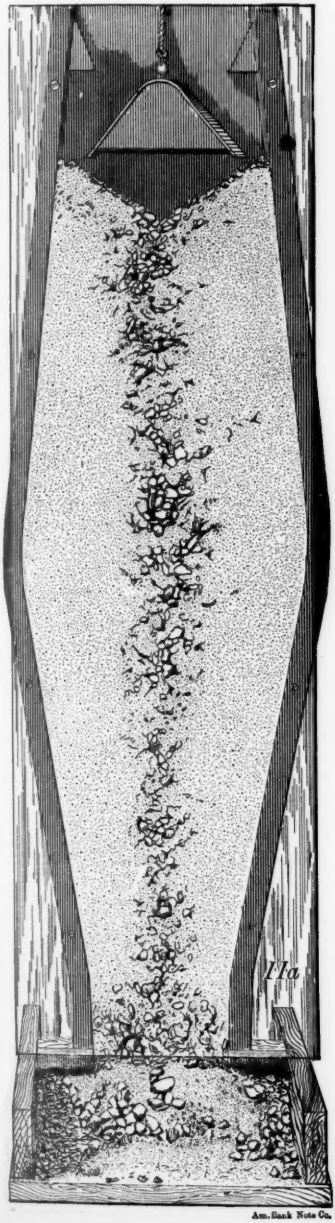


FIG. II *b*.

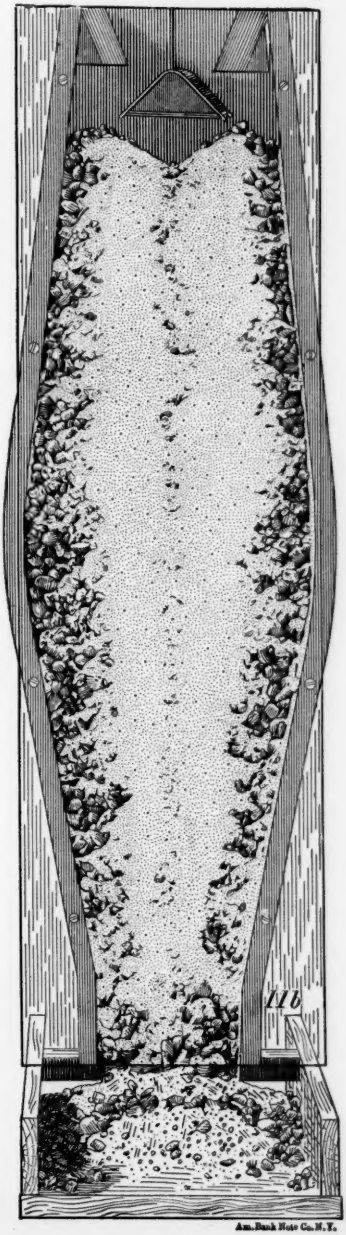


FIG. II *c*.

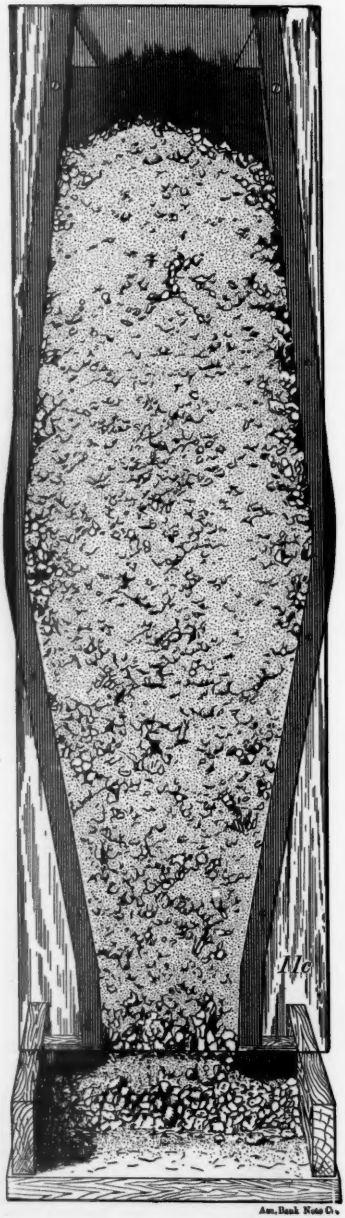


FIG. II *d*.

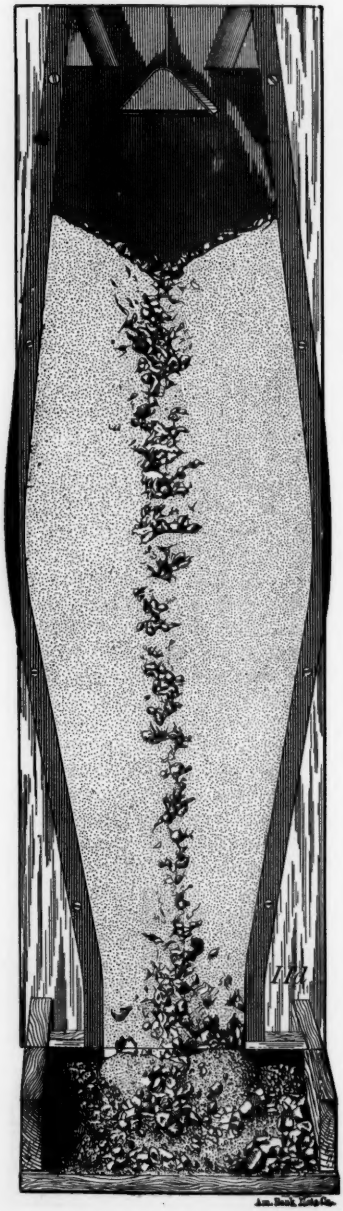


FIG. III *a*.

FIG. III *b*.

FIG. III *c*.

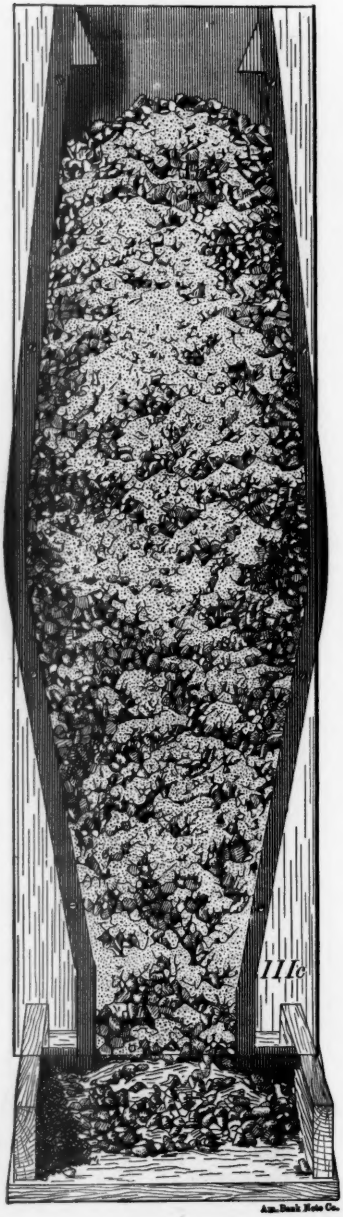


FIG. II *e*.

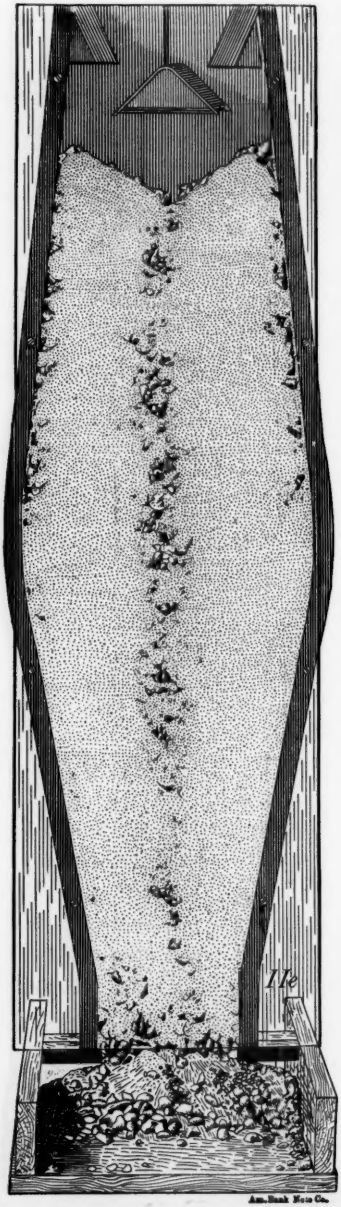


FIG. IV *a*.

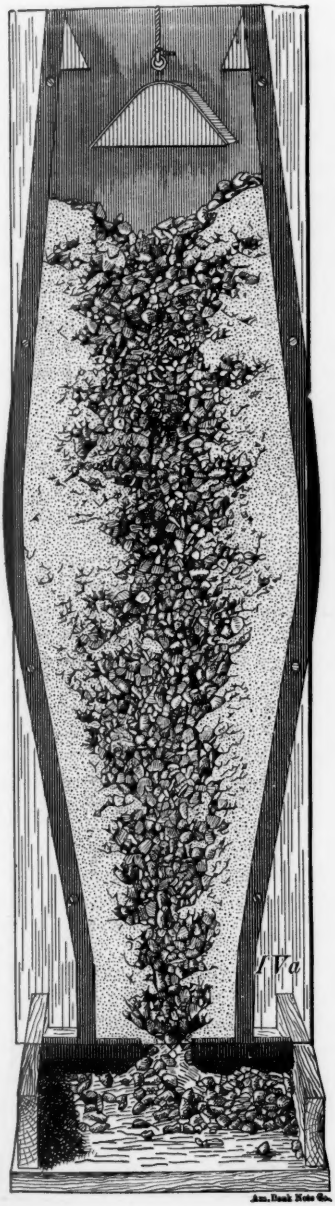


FIG. IV *b*.

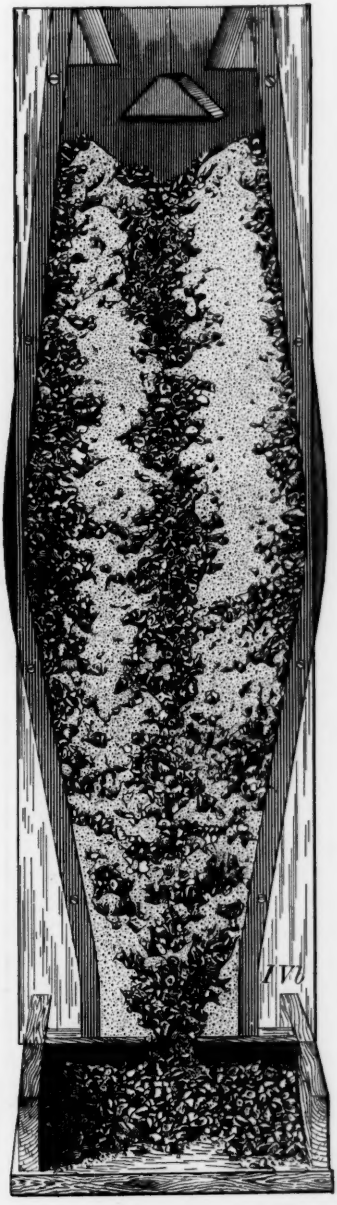


FIG. IV *c*.

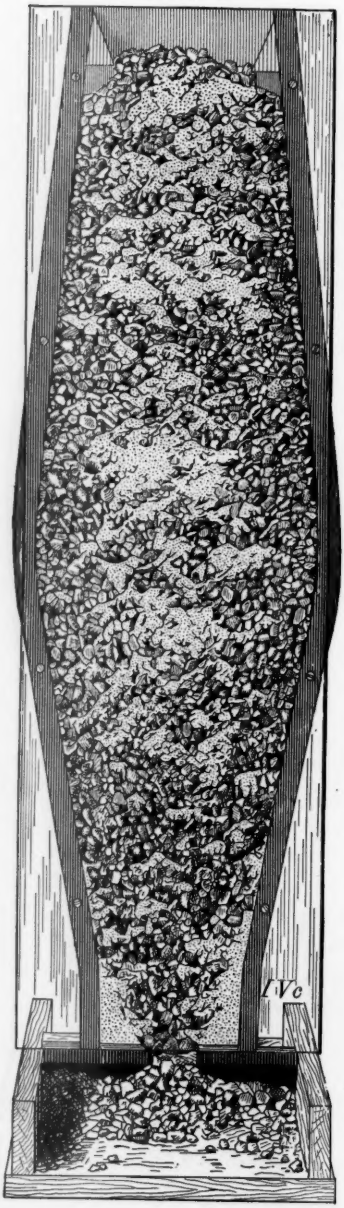


FIG. I *g*.

*FRICTION IN TOOTHED GEARING.**

BY GAETANO LANZA, S.B., C.E.

BEFORE presenting to the Society a paper so full of long equations, a brief statement will be made of the reasons that led to writing it, the assumptions on which the work is based, and the conclusions which seem to be warranted by the results of the computations. At the last annual meeting a paper on the efficiency of gear teeth, written by Prof. Reuleaux, was presented, and certain conclusions were drawn by him, one of which was that the work lost in friction is greater with the involute than with the epicycloidal form of tooth. This conclusion, as well as some of the others which he drew, was challenged by several of the members of the Society, who, making their computations by other methods, which they claimed were more exact, drew precisely the opposite conclusions from Prof. Reuleaux in the cases referred to, and also in other cases.

Moreover, Prof. Reuleaux's work, and also all the reasoning of his opponents, were based on purely theoretical grounds, and all the solutions presented were approximate. Indeed, all the solutions that the writer has seen in print, including those of Rankine, Hermann, and Moseley, are only approximations.

The present investigation was undertaken in order to determine, if possible, who, if any, were right in their conclusions.

While the writer is one who believes that this question can only be correctly answered by experiment, and that the experiments to be made will require very delicate measurements, and a more accurate experimental knowledge than is now possessed of the small errors of dynamometers, nevertheless, in the present paper, the reasoning is upon a purely theoretical basis; but no approximations have been made in the deduction of the formulæ for the work lost in friction, and the consequent efficiency of the gears when the friction of the journals in their bearings is not taken into account, this being the only case to be considered here. Hence the correctness of the results depends only upon the correctness of the assumptions made to start with, and these will now be discussed. Although the present paper deals fully as much with the practical cases where more than one pair of teeth are in action at once, as with the case where only one pair is in action at once, nevertheless, the assumptions made in this latter case will be discussed first,

*A paper presented to the American Society of Mechanical Engineers, at their meeting of November, 1887.

as that is the only one that is dealt with by Reuleaux and the other writers referred to above.

The efficiency of any pair of gears will be different for every different distribution of the pressure; and this will, when only one pair of teeth is in action at once, vary with the variation of the work done by the following, and the variation of the energy imparted to the driving-gear of the pair. To compute the efficiency of a given pair of gears for every possible distribution of the pressure that can occur in practice, would be an impossibility. Hence it would seem to be reasonable to adopt for our assumption in this regard one which should be fulfilled in a large number of practical cases, and we should then have an exact solution for these cases; whereas for gears under different conditions, the results of our computations can only be considered as approximate.

The assumption made in this paper, whether one pair or more than one pair of teeth are in action at once, is that the moment of the resistance is constant. This is equivalent to assuming that the work done by the following gear is constant. The above, and the ordinarily accepted laws of friction, are the only assumptions made in the case when only one pair of teeth is in action at once.

When two pairs of teeth are in action at once, it becomes necessary to make another assumption in addition to that of a constant moment of resistance, as this alone does not determine the way in which the work is divided between the two pairs of teeth in action. Formulæ are worked out in this paper, based upon each of the two following assumptions, viz.: —

(*a*) That the work done, and hence the moment of the resistance, is at all times equally divided between the two pairs of teeth in action.

(*b*) That the moment of the resistance is so divided between the two pairs in action as to cause them to wear equally; this being the assumption proposed by Mr. Hugo Bilgram.

When (*b*) is used a third one has to be made, and that adopted here is that the wear, as estimated in a normal direction, is proportional to the product of the normal pressure and the velocity of sliding.*

By way of a summary, we may say that when assumption (*a*) is used the assumptions made are the following: —

1. The ordinary friction theories.
2. The constancy of the moment of the resistance.
3. The equal division of the moment of the resistance between the two pairs of teeth in action.

*This the writer supposed to be in accordance with a suggestion made by Mr. Bilgram. See Trans. Am. Soc. Mech. Eng'rs, Vol. viii., 76.

On the other hand, when (*b*) is used the assumptions made are the following: —

1. The ordinary friction theories.
2. The constancy of the moment of the resistance.
3. The condition of equal wear.
4. The proportionality of the normal wear to the product of the normal pressure and the velocity of sliding.

Before discussing the results which will be found later in this paper, it will be observed that, after the deduction of formulæ for the efficiency of gears in the case where only one pair of teeth is in action at once, the numerical efficiencies of certain special cases of epicycloidal and involute teeth have been computed, which will be found on page 163; and it seems to the writer that from a consideration of the formulæ and examples we may fairly conclude,—

1. That whether the epicycloidal or the involute form of tooth is the most efficient, depends upon the proportions used for each, and that either one may be more efficient than the other, according to the way these proportions are chosen.
2. That the efficiency of involute gears is not, as has been claimed by Mr. George B. Grant and others, independent of the obliquity; the reason why they have drawn an erroneous conclusion in this regard being the approximations which they have introduced into their work.
3. The differences between the efficiencies deduced for the four cases computed are so small, that it seems to the writer that they could be easily masked by any of the following matters: —

(*a*) By any variation in the coefficient of friction.
(*b*) By any inaccuracy in our friction theories.
(*c*) By any irregularity in the distribution of the pressure.

4. Hence follows the conclusion already stated, that the question can only be correctly answered by experiment.

Case when more than one pair of teeth are in action at once. — As to a choice between the two assumptions of equal division of work or of equal wear, we must observe: —

1. The formulæ based upon the first are identical with those deduced for the case where only one pair of teeth is in action at once, and they are far simpler than those based upon the last assumption.
2. Although the arguments advanced by Mr. Hugo Bilgram in favor of the equal wear theory possess some plausibility, it is not at all certain that the wear is equal.
3. The assumption that the normal wear is proportional to the product of the normal pressure and the velocity of sliding which we make

use of when we adopt the equal-wear theory, is in accord with what may be called the more modern theory of friction; and although this last is more popular than the older theory at present, nevertheless it is known that in certain cases of pivot friction it leads to conclusions which are manifestly absurd.

4. Finally, an inspection of the table on page 170, in which are determined the efficiencies of what the writer believes to be the Brown & Sharpe system of epicycloidal and involute gears, will show such small differences between the two forms, that, as it seems to the writer, they might be easily masked by any of the matters mentioned before as liable to mask the differences when only one pair of teeth is in action at once.

5. The smallness of the differences between the results of making the computations by the two different assumptions, would warrant our using the first, on account of its greater simplicity, whenever it is desired merely to determine the efficiency of the gears; and, as has been already stated, it does not seem to the writer that we are warranted in making comparisons between different kinds of gearing on the basis of such small differences as those in this table. The formulæ will now be deduced, and the tables of results given. In the case, however, of the assumption of equal wear, the formulæ will be given, but their deduction will be omitted on account of its length.

In this discussion the friction of the journals on their bearings is left out of consideration, and the weight of the gears is assumed to be without appreciable influence, and the notation used is substantially that of Moseley.

Let the centre of the driver be B, and that of the follower, C. Let P_1 be the driving force, its leverage being

$$BD = a_1.$$

Let P_2 be the resistance, its leverage being

$$CE = a_2.$$

ONLY ONE PAIR OF TEETH IN CONTACT AT ONE TIME.

We will first consider the case when only one pair of teeth is in contact at one time. The resultant pressure between the two teeth in contact (see Fig. 1) must make an angle φ (the angle of friction) with the common normal at the point of contact; hence its line of direction will be NM, where the angle $APN = \varphi$. If we denote its magnitude by R, we must have, from the conditions of equilibrium, if we let $BM = m_1$ and $CN = m_2$,

$$P_1 a_1 = R m_1, \tag{1}$$

$$P_2 a_2 = R m_2. \tag{2}$$

Hence,
$$\frac{P_1 a_1}{P_2 a_2} = \frac{m_1}{m_2}. \tag{3}$$

And these equations must always hold, whatever the form of the teeth.

We must next determine the values of m_1 and m_2, observing that the expressions which represent them during the approach will be different

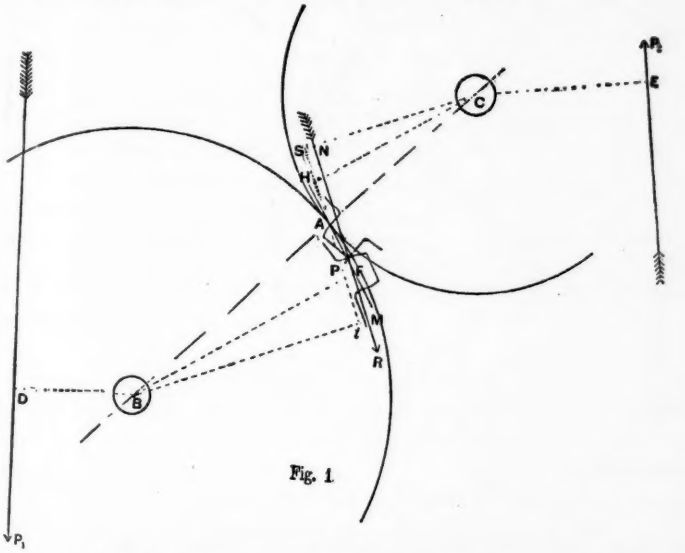


Fig. 1

from those which represent them during the recess. Their values during the approach may be deduced as follows (see Fig. 1):—

Let $BA = r_1$, $CA = r_2$, $AP = \lambda$, this being the length of the line joining the pitch point with the point of contact, and let the angle $PAB = \Theta$. Then we have from the figure,

$$m_1 = BM = Bt + tM = r_1 \sin BAt + \lambda \sin PAt;$$

or
$$m_1 = r_1 \sin (\Theta - \varphi) + \lambda \sin \varphi. \tag{4}$$

$$m_2 = CN = CS - SN = r_2 \sin SAC - \lambda \sin APN;$$

or
$$m_2 = r_2 \sin (\Theta - \varphi) - \lambda \sin \varphi. \tag{5}$$

On the other hand, we shall have, during the arc of recess (see Fig. 2), the equations,

$$m_1 = r_1 \sin (\Theta + \varphi) + \lambda \sin \varphi. \tag{6}$$

$$m_2 = r_2 \sin (\Theta + \varphi) - \lambda \sin \varphi. \tag{7}$$

By substituting these values of m_1 and m_2 in equations (1), (2), and (3), they will assume the following forms, which hold, whatever be the forms of the teeth.

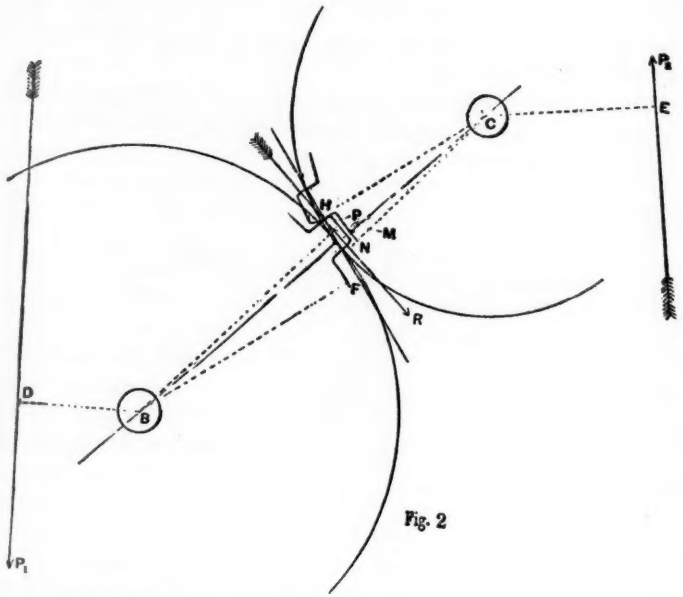


Fig. 2

Approach :—

$$P_1a_1 = Rr_1 \sin(\Theta - \varphi) + R\lambda \sin \varphi. \tag{8}$$

$$P_2a_2 = Rr_2 \sin(\Theta - \varphi) - R\lambda \sin \varphi. \tag{9}$$

$$\therefore \frac{P_1a_1}{P_2a_2} = \frac{r_1}{r_2}\left\{\frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin(\Theta - \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin(\Theta - \varphi)}}\right\}. \tag{10}$$

Recess :—

$$P_1a_1 = Rr_1 \sin(\Theta + \varphi) + R\lambda \sin \varphi. \tag{11}$$

$$P_2a_2 = Rr_2 \sin(\Theta + \varphi) - R\lambda \sin \varphi. \tag{12}$$

$$\therefore \frac{P_1a_1}{P_2a_2} = \frac{r_1}{r_2}\left\{\frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin(\Theta + \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin(\Theta + \varphi)}}\right\}. \tag{13}$$

Let now ψ denote the angle turned through by the follower since the

time when the point of contact was on the line BC; then will the corresponding angle turned through by the driver be

$$\frac{r_2}{r_1}\psi,$$

and we shall also have

$d\psi$ = elementary angle turned through by the follower;

$\frac{r_2}{r_1}d\psi$ = elementary angle turned through by the driver.

Now let

ψ_1 = angle turned through by follower during approach.
ψ_2 = angle turned through by follower during recess.
U_1 = work done by driver during approach.
U_2 = work transmitted by follower during approach.
U_3 = work done by driver during recess.
U_4 = work transmitted by follower during recess.

Then we can readily obtain from (10) and (13) the following equations.

Approach:

$$U_1=\int_0^{\psi_1}P_1a_1\frac{r_2}{r_1}d\psi=\int_0^{\psi_1}P_2a_2\left\{\frac{1+\frac{\lambda\sin\varphi}{r_1\sin(\Theta-\varphi)}}{1-\frac{\lambda\sin\varphi}{r_2\sin(\Theta-\varphi)}}\right\}d\psi. \qquad (14)$$

Recess:

$$U_3=\int_0^{\psi_2}P_1a_1\frac{r_2}{r_1}d\psi=\int_0^{\psi_2}P_2a_2\left\{\frac{1+\frac{\lambda\sin\varphi}{r_1\sin(\Theta+\varphi)}}{1-\frac{\lambda\sin\varphi}{r_2\sin(\Theta+\varphi)}}\right\}d\psi. \qquad (15)$$

and these two equations are entirely general; *i. e.*, they always hold whatever the form of the teeth, and whatever assumption be made in regard to the way in which the pressure varies at different points in the path of contact.

Under the assumption of a constant moment of resistance, they become, —

Approach:

$$U_1=P_2a_2\int_0^{\psi_1}\left\{\frac{1+\frac{\lambda\sin\varphi}{r_1\sin(\Theta-\varphi)}}{1-\frac{\lambda\sin\varphi}{r_2\sin(\Theta-\varphi)}}\right\}d\psi. \qquad (16)$$

Recess:

$$U_3 = P_2 a_2 \int_0^{\psi_2} \left\{ \frac{1 + \frac{\lambda \sin \varphi}{r_1 \sin (\Theta + \varphi)}}{1 - \frac{\lambda \sin \varphi}{r_2 \sin (\Theta + \varphi)}} \right\} d\psi. \qquad (17)$$

and then we have also

$$U_2 = P_2 a_2 \psi_1,$$
$$U_4 = P_2 a_2 \psi_2,$$

and from these equations we can find the efficiency

$$\frac{U_2 + U_4}{U_1 + U_3}.$$

EPICYCLOIDAL TEETH.

The only cases that will be discussed here are those where the same describing circle is used for the approach and recess.

Let r = radius of the describing circle, and let

$$\frac{r_2}{2r} = e.$$

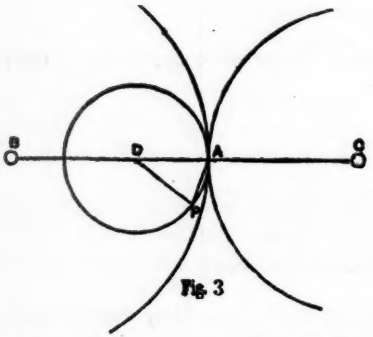


Fig. 3

Then we shall have, as shown in the figure, where

$$BA = r_1,\ CA = r_2,$$
$$DA = r,\ AP = \lambda.$$

Arc $AP = r_2 \psi$.

$\therefore$ angle $ADP = \frac{r_2}{r} \psi$,

and $\lambda = AP = 2r \sin \left(\frac{ADP}{2} \right)$

$= 2r \sin e\psi$.

Also it will be evident from the figure that

$$\Theta = DAP = \frac{\pi}{2} - e\psi.$$

$$\Theta - \varphi = \frac{\pi}{2} - (e\psi + \varphi).$$

$$\Theta + \varphi = \frac{\pi}{2} - (e\psi - \varphi).$$

If we make these substitutions in equations (16) and (17), and make the necessary integrations, we shall obtain

Approach:

$$U_1 = \frac{P_2 a_2}{e}\left\{\frac{\left[1+\left(1-\frac{2r}{r_1}\right)\left(1+\frac{2r}{r_2}\right)\tan^2\varphi\right]e\psi_1}{1+\left(1+\frac{2r}{r_2}\right)\tan^2\varphi}\right.$$

$$\left.\frac{-\left(\frac{2r}{r_1}+\frac{2r}{r_2}\right)\tan\varphi\log_e\left(\cos e\psi_1-\left[1+\frac{2r}{r_2}\right]\tan\varphi\sin e\psi_1\right)}{1+\left(1+\frac{2r}{r_2}\right)^2\tan^2\varphi}\right\}. \quad (18)$$

Recess:

$$U_3 = \frac{P_2 a_2}{e}\left\{\frac{\left[1+\left(1+\frac{2r}{r_1}\right)\left(1-\frac{2r}{r_2}\right)\tan^2\varphi\right]e\psi_2}{1+\left(1-\frac{2r}{r_2}\right)^2\tan^2\varphi}\right.$$

$$\left.\frac{-\left(\frac{2r}{r_1}+\frac{2r}{r_2}\right)\tan\varphi\log_e\left(\cos e\psi_2+\left[1-\frac{2r}{r_2}\right]\tan\varphi\sin e\psi_2\right)}{1+\left(1-\frac{2r}{r_2}\right)^2\tan^2\varphi}\right\}. \quad (19)$$

Having found the values of U_1 and U_3 as above, we can readily deduce the efficiency whose value is

$$\frac{U_2+U_4}{U_1+U_3}.$$

When $r_1=\infty$, or the driver becomes a rack, we merely substitute its value; certain terms drop out, and there is no indetermination.

When $r_2=\infty$, or the follower becomes a rack, we must make certain substitutions, as follows: —

Let $A_1 = r_2\psi_1 =$ arc of approach.

Let $A_2 = r_2\psi_2 =$ arc of recess.

Observe that in this case $a_2 = r_2$.

Then we may write

$$\frac{P_2 a_2}{e} = 2Pr, \qquad e\psi_1 = \frac{A_1}{2r}, \qquad e\psi_2 = \frac{A_2}{2r}, \qquad \frac{r}{r_2} = \frac{2r}{r_2} = 0.$$

If these substitutions be made, we have the formula which applies when the follower is a rack.

Before making any numerical computations under this case, we will first deduce the corresponding formulæ for involutes.

INVOLUTE TEETH.

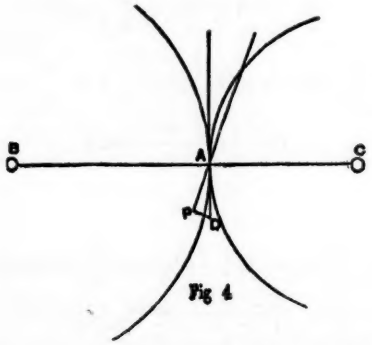


Fig 4

Let angle $PAD = \eta =$ the obliquity. Then if P is the point of contact corresponding to the angle ψ turned through by the follower, we shall have

$$\lambda = AP = r_2\psi \cos \eta,$$

$$\Theta = PAB = \frac{\pi}{2} - \eta,$$

$$\Theta - \varphi = \frac{\pi}{2} - (\eta + \varphi).$$

$$\Theta + \varphi = \frac{\pi}{2} - (\eta - \varphi).$$

Hence from equations (16) and (17) we obtain:—

Approach:

$$U_1 = -P_2 a_2 \left\{ \frac{r_2}{r_1}\psi_1 + \left(1 + \frac{r_2}{r_1}\right) \frac{\cos(\eta+\phi)}{\cos\eta \sin\phi} \log_e \left(1 - \frac{\cos\eta \sin\phi}{\cos(\eta+\phi)}\psi_1\right)\right\}. \quad (20)$$

Recess:

$$U_3 = -P_2 a_2 \left\{ \frac{r_2}{r_1}\psi_2 + \left(1 + \frac{r_2}{r_1}\right) \frac{\cos(\eta-\phi)}{\cos\eta \sin\phi} \log_e \left(1 - \frac{\cos\eta \sin\phi}{\cos(\eta-\phi)}\psi_2\right)\right\}. \quad (21)$$

When $r_1 = \infty$, or the driver becomes a rack, we merely substitute this value, certain terms drop out, and there is no indetermination.

When $r_2 = \infty$, or the follower becomes a rack, equations (20) and (21) would become indeterminate. To remove the indetermination put $a_2 = r_2$, and for ψ write $\frac{A}{r_2}$, where $A = r_2\psi$, and introduce A as the independent variable, and put $r_2 = \infty$ before making the integrations.

We thus obtain

Approach:

$$U_1 = P_2 A_1 \left\{ 1 + \frac{A_1}{2r_1} \frac{\cos\eta \sin\varphi}{\cos(\eta + \varphi)} \right\}. \tag{22}$$

Recess:

$$U_3 = P_2 A_2 \left\{ 1 + \frac{A_2}{2r_1} \frac{\cos\eta \sin\varphi}{\cos(\eta - \varphi)} \right\}. \tag{23}$$

EXAMPLES.

In the following four examples we will assume a pair of 30-tooth gears, the diameter of whose pitch circles is 15 inches, the addenda being so taken as to give one tooth contact. This will give us in all four cases.

$r_1 = r_2 = 7.''5$; $\psi_1 = \psi_2 = \frac{\pi}{30}$. Assume in all cases $\tan\varphi = 0.1$.

I. Given $r = 1.''875$, the teeth being epicycloidal. Find the efficiency.

Result, efficiency $= 0.9895$.

II. Given $r = 3.''75$, the teeth being epicycloidal. Find the efficiency.

Result, efficiency $= 0.9886$.

III. Given $\eta = 14°$, the teeth being involutes. Find the efficiency.

Result, efficiency $= 0.9901$.

IV. Given $\eta = 18°$, the teeth being involutes. Find the efficiency.

Result, efficiency $= 0.9894$.

TWO OR MORE PAIRS OF TEETH IN CONTACT.

Let P_1, P_2, a_1, a_2, have the same meanings as before.

Let R denote the resultant pressure between the forward pair of teeth, R_1, that between the second, etc.

Let m_1', m_1'', etc., be the perpendiculars from B on R, R_1, etc., respectively.

Let m_2', m_2'', etc., be the perpendiculars from C on R, R_1, etc., respectively.

Then we must have the equations

$$P_1 a_1 = R m_1' + R_1 m_1'' + \text{etc.} \tag{24}$$

$$P_2 a_2 = R m_2' + R_1 m_2'' + \text{etc.} \tag{25}$$

And, moreover, the pressures R, R_1, etc., make angles φ with the normals at the points of contact, as shown in the figure. In order to work out the efficiency of a pair of gears where more than one pair of teeth

are in contact at the same time, it is necessary to make some assumption in regard to the way in which the work is divided between the different pairs of teeth.

ASSUMPTION OF EQUAL DIVISION OF WORK.

Let

a = pitch angle,
ψ_1 = angle of approach,
ψ_2 = angle of recess,

all measured on the follower; and let $\psi_1 \leqq a$, $\psi_2 \leqq a$, so as to include all cases where two pairs of teeth are in contact simultaneously, either all or a part of the time.

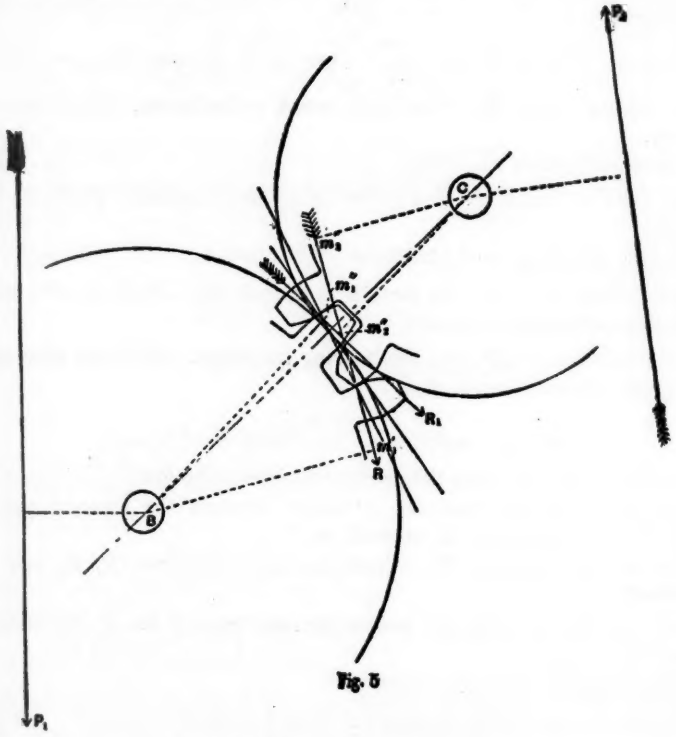


Fig. 5

The assumption of equal division of work gives:

$$Rm_2' = R_1m_2'' = \frac{P_2a_2}{2}$$

$$\therefore P_2a_2 = 2Rm_2' = 2R_1m_2''.$$

Now, we shall ascertain the work required to be exerted on the driver during the action of one given pair of teeth, by separating the whole period $\psi_1 + \psi_2$ of contact of one tooth on the follower into the several parts, when two pairs are engaged, and when one pair alone are engaged, and then determine the work to be exerted on the driver corresponding to each of these parts. Then, adding them all together, we shall obtain the entire work corresponding to the angle $\psi_1 + \psi_2$.

Now, let us call the angle made by a tooth with the line of centers, the angle made with the line of centers, by the radius of that point of the face of the tooth that lies on the pitch circle. Then we shall have, in the motion of any one tooth, the following four stages; moreover, to fix the ideas, we will assume the path of approach to lie to the left, and that of recess to the right, of the pitch point: —

1°. From angle ψ_1 to angle $\alpha - \psi_2$, to the left of the pitch point the tooth is in approach while the tooth ahead of it is in recess.

2°. From $\alpha - \psi_2$ to 0 the tooth is in approach and is acting alone.

3°. From 0 to $\alpha - \psi_1$ to the right of the pitch point the tooth is in recess and is acting alone.

4°. From $\alpha - \psi_1$ to ψ_2 the tooth is in recess with the tooth behind it in approach.

Moreover, we have also:

5°. The corresponding angles for the forward tooth corresponding to 1° are

$$\alpha - \psi_1 \text{ and } \psi_2.$$

6°. The corresponding angles for the rear tooth corresponding to 4° are

$$\psi_1 \text{ and } \alpha - \psi_2.$$

Now the angle from ψ_1 to $\alpha - \psi_2$ of the approach, and also the angle from $\alpha - \psi_1$ to ψ_2 of the recess, is passed over by two pairs of teeth successively, each pair having the moment of resistance $\frac{P_2 a_2}{2}$, which is equivalent to having one pair of teeth pass over these angles with the moment $P_2 a_2$. The remainder of the angle $\psi_1 + \psi_2$, *i. e.*, that from $\alpha - \psi_2$ to 0 of the approach and that from 0 to $\alpha - \psi_1$ of the recess by only a single pair of teeth having the moment of resistance $P_2 a_2$. Hence the work done by the driver while the follower turns through the angle $\psi_1 + \psi_2$ is

$$U_1 = P_2 a_2 \int_0^{\psi_2} \left\{ \frac{1 + \frac{\lambda \sin \varphi}{r_1 \sin (\Theta - \varphi)}}{1 - \frac{\lambda \sin \varphi}{r_2 \sin (\Theta - \varphi)}} \right\} d\psi$$

$$+ P_2 a_2 \int_0^{\psi_1} \left\{ \frac{1 + \frac{\lambda \sin \varphi}{r_1 \sin (\Theta + \varphi)}}{1 - \frac{\lambda \sin \varphi}{r_2 \sin (\Theta + \varphi)}} \right\} d\psi.$$

This is the same equation (in form) that was deduced for only one pair of teeth in action at once, and it is equally true (in form) for any number of pairs in action simultaneously. Hence we may say that the following are the formulæ for the work to be exerted on the driver while the follower traverses the angle $\psi_1 + \psi_2$.

EPICYCLOIDS.

$$U_1 =$$

$$\frac{P_2 a_2}{e} \left\{ \frac{\left[1 + \left(1 - \frac{2r}{r_1}\right)\left(1 + \frac{2r}{r_2}\right)\tan^2\varphi\right] e\psi_1}{1 + \left(1 + \frac{2r}{r_2}\right)^2 \tan^2\varphi} \right.$$

$$\left. - \frac{\left(\frac{2r}{r_1} + \frac{2r}{r_2}\right)\tan\varphi \log_e \left[\cos e\psi_1 - \left(1 + \frac{2r}{r_2}\right)\tan\varphi \sin e\psi_1\right]}{1 + \left(1 + \frac{2r}{r_2}\right)^2 \tan^2\varphi} \right\}$$

$$+ \frac{P_2 a_2}{e} \left\{ \frac{\left[1 + \left(1 + \frac{2r}{r_1}\right)\left(1 - \frac{2r}{r_2}\right)\tan^2\varphi\right] e\psi_2}{1 + \left(1 - \frac{2r}{r_2}\right)^2 \tan_2\varphi} \right.$$

$$\left. - \frac{\left(\frac{2r}{r_1} + \frac{2r}{r_2}\right)\tan\varphi \log_e \left[\cos e\psi_2 + \left(1 - \frac{2r}{r_2}\right)\tan\varphi \sin e\psi_2\right]}{1 + \left(1 - \frac{2r}{r_2}\right)^2 \tan^2\varphi} \right\}$$

INVOLUTES.

$$U_1 = -P_2 a_2 \left\{ \frac{r_2}{r_1}\psi_1 + \left(1 + \frac{r_2}{r_1}\right)\frac{\cos(\eta + \phi)}{\cos\eta \sin\phi} \log_e \left(1 - \frac{\cos\eta \sin\phi}{(\cos\eta + \phi)}\psi_1\right) \right\}$$

$$- P_2 a_2 \left\{ \frac{r_2}{r_1}\psi_2 + \left(1 + \frac{r_2}{r_1}\right)\frac{\cos(\eta - \phi)}{\cos\eta \sin\phi} \log_e \left(1 - \frac{\cos\eta \sin\phi}{\cos(\eta - \phi)}\psi_2\right) \right\} \cdot \quad (27)$$

these formulæ being the same as those given when only one pair of teeth is in action at once.

When either the driver or the follower is a rack, the same course is to be pursued as was indicated in the case of one-tooth contact. It is also to be observed that when these formulæ are used for more than one pair of teeth in action at once, they are based on the assumption that the moment of the resistance is at all times divided equally between the separate teeth.

ASSUMPTION OF EQUAL WEAR.

In this case only the resulting formulæ will be given for epicycloidal and involute gearing, the deduction being omitted.

Let U_1' denote the work done by the driver during that part of the contact of a given pair of teeth while two pairs are in action; and U_1'' that done by the driver while only one pair is in action.

Then we shall have

EPICYCLOIDAL TEETH.

Let

$$a = \frac{2r \tan \varphi \sec e\alpha}{r_1 \tan e\alpha - 2r \tan \varphi}, \text{ and } b = \frac{2r \tan \varphi \sec e\alpha}{r_2 \tan e\alpha + 2r \tan \varphi},$$

Then will

$$U_1' = 2P_2a_2 \frac{r_2}{r_1}(a - \psi_1 - \psi_2) + 2\frac{P_2a_2\, r_2(a+b)}{er_1 a\sqrt{1-b^2}} \left\{ \tan^{-1}\left[\left(\frac{1+b}{1-b}\right)^{\frac{1}{2}} \tan\left(e\psi_1 - \frac{e\alpha}{2}\right)\right] + \tan^{-1}\left[\left(\frac{1+b}{1-b}\right)^{\frac{1}{2}} \tan\left(e\psi_2 - \frac{e\alpha}{2}\right)\right]\right\}. \quad (28)$$

In order to obtain the whole of the work to be performed upon the driver during the entire action of the tooth selected, we must add to (28) the work corresponding to the two middle periods when there is only one pair of teeth in action. This latter is to be found from the following formula:

$$U_1'' = P_2a_2 \int_0^{\alpha-\psi_2} \left\{ \frac{1 + \frac{\lambda \sin \varphi}{r_1 \sin(\Theta - \varphi)}}{1 - \frac{\lambda \sin \varphi}{r_2 \sin(\Theta - \varphi)}} \right\} d\psi + P_2a_2 \int_0^{\alpha-\psi_1} \left\{ \frac{1 + \frac{\lambda \sin \varphi}{r_2 \sin(\Theta + \varphi)}}{1 - \frac{\lambda \sin \varphi}{r_2 \sin(\Theta + \varphi)}} \right\} d\psi, \quad (29)$$

by making the proper substitutions. Then we have for the total work to be done upon the driver, while the follower describes the angle $\psi_1 + \psi_2$, the expression

$$U_1' + U_1''.$$

Case when a rack is the driver. When $r_1 = \infty$, (28) would become indeterminate. If, however, we observe that

$$r_1 a = \frac{2r \tan \varphi \sec e\alpha}{\tan e\alpha - \frac{2r}{r_1} \tan \varphi}$$

and that when $r_1 = \infty$, this becomes

$$r_1 a = \frac{2r \tan \varphi}{\sin e\alpha},$$

and if we substitute this for $r_1 a$ in the denominator of (28) all the indetermination disappears.

The mode of removing the indetermination from (29) is the same as was previously explained.

Case when a rack is the follower. When $r_2 = \infty$, we need to make the following substitutions in the differential equation from which (28) is derived.

Let A_1 = arc of approach, A_2 = arc of recess, A = arc whose angle measured on the follower is ψ, and let p = the pitch:

Then

$$\psi = \frac{A}{r_2},\ \psi_1 = \frac{A_1}{r_2},\ \psi_2 = \frac{A_2}{r_2},\ d\psi = \frac{dA}{r_2},\ \alpha = \frac{p}{r_2},\ a_2 = r_2,\ e\psi = \frac{A}{2r},\ e\alpha = \frac{p}{2r}$$

Then

$$U_1' = 2P_2 r \left\{ \frac{2r \tan \varphi}{r_1 \tan e\alpha} - 1 \right\} \int_{p-A_2}^{A_1} \left\{ 1 + a \cos \left(\frac{p}{2r} - \frac{A}{r} \right) \right\} d \left(\frac{p}{2r} - \frac{A}{r} \right).$$

$$\therefore U_1' = 2P_2 r \left\{ \frac{2r \tan \varphi}{r_1 \tan e\alpha} - 1 \right\} \left\{ \frac{p - A_1 - A_2}{r} + a \left[\sin \left(\frac{p}{2r} - \frac{A_1}{r} \right) - \sin \left(\frac{A_2}{r} - \frac{p}{2r} \right) \right] \right\}. \quad (30)$$

The changes to be made in equation (29) have already been explained.

INVOLUTE TEETH.

Let

$$a = \frac{\cos(\eta - \varphi)}{\cos(\eta + \varphi)}, \quad b = 2 \frac{r_2}{r_1} \frac{\cos \eta \sin \varphi}{\cos(\eta + \varphi)},$$

$$c = \frac{2 \cos \eta \sin \varphi}{\cos(\eta + \varphi)}, \quad \text{and } e = \frac{a - 1 - c\alpha}{2c}.$$

Then

$$U_1' = 2P_2 a_2 \left(\frac{r_2}{r_1} \right) (\alpha - \psi_1 - \psi_2) + P_2 a_2 \frac{(a-1)\left(\frac{r_2}{r_1} + 1 \right)}{c}$$

$$\log_e\left\{\frac{(\psi_1+e)^2+\left(\frac{a}{c}-e^2\right)}{(a-\psi_2+e)^2+\left(\frac{a}{c}-e^2\right)}\right\}+2P_2a_2\,\frac{\left(\frac{r_2}{r_1}+1\right)\{a-e(a-1)\}}{c\left(\frac{a}{c}-e^2\right)^{\frac{1}{2}}}$$

$$\left\{\tan^{-1}\left(\frac{\psi_1+e}{\left(\frac{a}{c}-e^2\right)^{\frac{1}{2}}}\right)-\tan^{-1}\left(\frac{a-\psi_2+e}{\left(\frac{a}{c}-e^2\right)^{\frac{1}{2}}}\right)\right\}. \qquad (31)$$

The value of U_1'' can be deduced from equation (29) by making the suitable substitutions and integrations, or it can be derived directly from (27) by substituting $a-\psi_2$ for ψ_1, and $a-\psi_1$ for ψ_2.

Then will the total work to be exerted on the driver, while the follower describes the angle $\psi_1+\psi_2$, be

$$U_1'+U_1''.$$

When a rack drives, $r_1=\infty$, $\therefore b=0$, but no indetermination arises.

Case when a rack is the follower. To remove the indetermination, make the following substitutions in the differential equation from which (31) is derived: $a_2=r_2$, $\psi=\frac{A}{r_2}$, $d\psi=\frac{dA}{r_2}$, $\frac{b}{r_2}=b_1$, $a=\frac{p}{r_2}$.

Then when $r_2=\infty$, we have

$$U_1'=2P_2\int_{p-A_2}^{A_1}\left\{\frac{\frac{1}{A}+\frac{a}{p-A}+b_1}{\frac{1}{A}+\frac{a}{p-A}}\right\}dA=$$

$$2P_2\int_{p-A_2}^{A_1}\left\{\frac{p+(a-1)A+b_1A(p-A)}{p+(a-1)A}\right\}dA. \qquad (32)$$

Hence, by simple division, we obtain

$$U_1'=2P_2\left\{-\frac{b_1}{a-1}\int_{p-A_2}^{A_1}AdA+\left[\frac{b_1ap}{(a-1)^2}+1\right]\int_{p-A_2}^{A_1}dA\right.$$

$$\left.-\frac{b_1ap^2}{(a-1)_3}\int_{p-A_2}^{A_1}\frac{(a-1)\,dA}{p+(a-1)A}\right\}.$$

Hence, by making the integrations and inserting the limits, we have

$$U_1'=2P_2\left\{\frac{b_1}{a-1}\left[(p-A_2)^2-A_1^2\right]+\left[\frac{b_1ap}{(a-1)_2}+1\right](A_1+A_2-p)-\right.$$

$$\left.\frac{b_1ap^2}{(a-1)^3}\log_e\left(\frac{p+(a-1)A_1}{ap-(a-1)A_2}\right)\right\}. \qquad (32)$$

The manner of finding U_1'' has already been explained.

EPICYCLOIDS.

No. of Teeth on Follower.	ψ_1	15 Teeth on the Driver.			100 Teeth on the Driver.			Rack Driving.		
		ψ_2	Efficiency.		ψ_2	Efficiency.		ψ_2	Efficiency.	
			First Assumption.	Second Assumption.		First Assumption.	Second Assumption.		First Assumption.	Second Assumption.
15	17° 56′ 2.″2	17° 56′ 2.″3	.9685	.9697	20° 43′ 42.″0	.9801	.9828	21° 25′ 0.″2	.9823	.9850
50	6° 2′ 3.″2	5° 22′ 48.″7	.9782	.9796	6° 13′ 6.″6	.9913	.9932	6° 25′ 30.″2	.9944	.9951
100	3° 6′ 33.″3	2° 41′ 24.″3	.9792	.9806	3° 6′ 33.″3	.9932	.9953	3° 12′ 45.″0	.9959	.9975
Rack.			.9825	.9853		.9943	.9975			

INVOLUTES.

No. of Teeth on Follower.	ψ_1	15 Teeth on the Driver.			100 Teeth on the Driver.			Rack Driving.		
		ψ_2	Efficiency.		ψ_2	Efficiency.		ψ_2	Efficiency.	
			First Assumption.	Second Assumption.		First Assumption.	Second Assumption.		First Assumption.	Second Assumption.
15	20° 3′ 2.″3	20° 3′ 2.″3	.9654	.9704	27° 53′ 35.″0	.9754	.9870	31° 30′ 56.″9	.9764	*
50	7° 39′ 44.″9	6° 0′ 54.″7	.9659	.9823	8° 22′ 4.″2	.9895	*	9° 27′ 16.″2	.9924	*
100	4° 11′ 2.″1	3° 0′ 27.″3	.9758	.9861	4° 11′ 2.″1	.9928	*	4° 49′ 38.″6	.9961	*
Rack.			.9767	*		.9961	*			

EXAMPLES OF INTERCHANGEABLE GEARS.

There will now be given in tabular form the results of using the formulæ that have been deduced to determine the efficiency of an interchangeable set of epicycloidal gears where the addendum is equal to pitch divided by π, the diameter of the describing circle being one half the diameter of the fifteen-tooth pinion; also the efficiency of an interchangeable set of involute gears with the same addendum as above stated, and with an obliquity of 14° 30′, these being usual systems. The first thing to be done before using the formulæ is to determine the angle of approach and that of recess, as measured on the follower, and these can be very easily determined by computation.

In the following tables the values of these angles will be given in every case except when the follower is a rack; for, in that case, we have to use the arcs instead of the angles, or else use the angles as measured on the driver. The efficiencies given in the tables have been determined on the assumption of a coefficient of friction equal to $\frac{1}{10}$, and on each of the two assumptions in regard to the distribution of pressure that have been already referred to.

In the cases marked with an asterisk, $\psi_1 + \psi_2 > 2a$; and hence three pairs of teeth are in contact at once during a part of the travel. When, therefore, we use the second assumption, the formulæ already developed on the assumption of equal wear do not apply, and it would be necessary to develop formulæ for three-tooth contact in order to determine the efficiencies, if these are to be computed by the second assumption.

EFFICIENCY OF A TRAIN OF GEARS.

When we wish to determine the efficiency of a train of gears, it is not correct merely to determine the efficiency of each pair by the preceding formulæ, and to multiply these efficiencies together. An approximation to the truth might be obtained by such a proceeding, but not a correct result; for the efficiency of any pair varies with the mode of variation of the moment of the resistance, and this mode of variation will be different for each successive gear in the train.

Hence we must use the above-described process only while we are dealing with an elementary angle $d\psi$, and make our integration after we have multiplied the elementary efficiencies.

To illustrate this method let us assume that we have a train of four gears, of which No. 1 is the driver and No. 4 the follower, and that the arc of action is, in every case, equal to one pitch.

Let us number them successively, beginning with the driver. Let P_1a_1 denote the driving moment exerted on gear No. 3, and P_2a_2, as

before, the moment of the resistance of gear No. 4. Let R be the pressure between the teeth of Nos. 3 and 4, R_1 that between Nos. 2 and 3, and R_2 that between Nos. 1 and 2.

Let the radii of the pitch circles of the four gears in their order be r_1, r_2, r_3, and r_4. Let m_1 and m_2 have the same meanings as before with reference to Nos. 3 and 4, m_1', m_2' be the corresponding quantities for Nos. 2 and 3, and m_1'', m_2'' for Nos. 1 and 2. Then we must have the equations

$$\begin{aligned} Rm_2 &= P_2a_2, \\ R_1m_2' &= Rm_1, \\ R_2m_2'' &= R_1m_2', \\ P_1a_1 &= R_2m_1''. \end{aligned}$$

By eliminating the R's we obtain

$$P_1a_1 = \frac{m_1''}{m_2''}\frac{m_1'}{m_2'}\frac{m_1}{m_2}P_2a_2.$$

If, now, $d\psi$ denote the elementary angle turned through by the follower, then will that turned through by the driver be

$$\frac{r_4}{r_1}d\psi.$$

Hence we have $$dU_1 = P_1a_1\frac{r_4}{r_1}d\psi = \frac{r_4}{r_1}\frac{m_1''}{m_2''}\frac{m_1'}{m_2'}\frac{m_1}{m_2}P_2a_2d\psi,$$

and hence $$U_1 = \frac{r_4}{r_1}\int\frac{m_1''}{m_2''}\frac{m_1'}{m_2'}\frac{m_1}{m_2}P_2a_2d\psi,$$

and by this means we can find the efficiency of the train.

The integration may become very complex, and the quantity to be integrated will be different for every different location of the axles. Thus, when the approach of a pair of teeth of Nos. 3 and 4 is just beginning, the pair in action between Nos. 2 and 3 may be just beginning their approach, just finishing their recess, or anywhere else, according to the relative positions of the axles. Hence it would hardly seem to be worth while to work out any special cases here.

When the arc of contact is greater than one pitch the complexity is increased, and the uncertainty is no less. Hence, when we desire to know the efficiency of a train of gears it would seem to be best to obtain it by multiplying together the efficiencies of the different pairs, but to bear in mind that we thus obtain only an approximate solution, and not to draw any conclusions from the result that cannot fairly be drawn, in view of the fact that such lack of exactness exists and has a tolerably wide range.

ON THE SOLUBILITY OF MAGNESIUM-AMMONIUM PHOSPHATE IN ALCOHOL.

BY ALFRED J. WAKEMAN, PH.B.

IN Rose's Handbuch der Analytischen Chemie, sixth edition, 1871, it is said, under the precipitation of magnesium by sodium phosphate, that the precipitate of magnesium-ammonium phosphate is entirely insoluble in water containing one fourth of its bulk of ammonia water, sp. gr. 0.96.

Under the determination of phosphoric acid by magnesium salts, it is said that water containing three per cent. of ammonia gas, dissolves only traces of the precipitated magnesium-ammonium phosphate, and that this solubility is reduced to less than half, if to this dilute ammonia is added one fourth of its volume of alcohol. Further, that the addition of alcohol favors the separation of the precipitate.

This statement of Rose with regard to the effect of alcohol in diminishing the solubility of magnesium-ammonium phosphate in ammoniacal water, seems to have been overlooked or disregarded by more recent analysts.

To determine whether there was any practical advantage in this use of alcohol, I have made, at the suggestion of Prof. T. M. Drown, a series of comparative analyses under the same conditions, except that some of the precipitates were washed with ammonia water, according to the usual practice, and others with ammonia water containing alcohol.

In all the experiments a solution of di-sodium-hydrogen phosphate was used, and the same amount, 50 c.c., was measured off at the same time into twelve beakers. As the object of the investigation was merely a comparison of results obtained by the use of the two solutions used for washing the precipitates, no attempt was made to determine the actual solubility of the precipitate in either.

A new solution was made for each series of experiments; consequently the results of the different series are not comparable with one another. In the First Series there was added to 50 c.c. of a solution of di-sodium-hydrogen phosphate, 8 c.c. of a solution of ammonium chloride, 15 c.c. of magnesia mixture, and 40 c.c. of ammonia (sp. gr. 0.96), making a bulk of 113 c.c.

All of the precipitates were washed on the filter fifteen times, with 7 c.c. of wash-water each time — a total bulk of 105 c.c. Six of the precipitates were washed with a mixture of three parts by volume of water, and one part of ammonia (sp. gr. 0.96), and six with this ammonia water, to which was added one part by volume of alcohol.

I.

Weight in Grammes of Magnesium Pyrophosphate.

Washed with Ammonia Water.	Washed with Ammonia and Alcohol.
0.0105	0.0104
0.0102	0.0103
0.0102	0.0102
0.0106	0.0103
0.0103	0.0103
0.0105	0.0103
0.01038 mean.	0.0103 mean.

In the Second Series nearly double the amount of phosphate was taken, the bulk of the solution being about the same. The precipitates were washed, as in the First Series.

II.

Weight in Grammes of Magnesium Pyrophosphate.

Washed with Ammonia Water.	Washed with Ammonia and Alcohol.
0.0198	0.0200
0.0199	0.0201
0.0198	0.0202
0.0200	0.0202
0.0200	0.0199
0.0199	0.0201
0.0199 mean.	0.02008 mean.

In the Third Series 25 c.c. of alcohol was added to six of the beakers in which the precipitation took place, and the subsequent treatment was the same as above.

III.

Weight in Grammes of Magnesium Pyrophosphate.

Washed with Ammonia Water.	Precipitated in Presence of Alcohol. Washed with Ammonia and Alcohol.
0.0199	0.0200
0.0199	0.0199
0.0200	0.0199
0.0201	0.0199
0.0202	0.0198
0.0198	0.0199
0.01998 mean.	0.0199 mean.

In the Fourth Series a still larger quantity of the salt was used. To 50 c.c. of this more concentrated solution was added 20 c.c. of ammonium chloride solution, 25 c.c. of magnesia mixture, and 50 c.c. ammonia water. The precipitates were washed on the filter fifteen times, with 7 c.c. of wash-water each time, as in the previous series

IV.

Weight in Grammes of Magnesium Pyrophosphate.

Washed with Ammonia Water.	Washed with Ammonia and Alcohol.
0.0741	0.0749
0.0741	0.0746
0.0739	0.0747
0.0735	0.0746
0.0738	0.0749
0.0737	0.0745
0.07385 mean.	0.0747 mean.

The Fifth Series was like the Fourth, except that 100 c.c. of alcohol was added to six beakers, in which the precipitation took place, the other conditions being the same.

V.

Weight in Grammes of Magnesium Pyrophosphate.

Washed with Ammonia Water.	Precipitated in Presence of Alcohol. Washed with Ammonia and Alcohol.
0.0751	0.0756
0.0751	0.0761
0.0752	0.0759
0.0747	0.0763
0.0752	0.0761
0.0751	(Lost.)
0.07506 mean.	0.0760 mean.

In the two following series no account was taken of the amount of wash-water used. In both cases they were certainly washed more than was absolutely necessary.

VI.

Weight in Grammes of Magnesium Pyrophosphate.

Washed with Ammonia Water.	Washed with Ammonia and Alcohol.
0.0747	0.0745
0.0736	0.0747
0.0739	0.0743
0.0732	0.0744
0.0738	0.0737
0.0732	0.0737
0.07373 mean.	0.07422 mean.

VII.

Weight in Grammes of Magnesium Pyrophosphate.

Washed with Ammonia Water.	Washed with Ammonia and Alcohol.
0.0737	0.0750
0.0737	0.0748
0.0735	0.0744
0.0743	0.0745
0.0744	0.0747
0.0743	0.0749
0.07398 mean.	0.07472 mean.

The foregoing determinations point to a slight advantage in the use of alcohol in diminishing the solubility of magnesium-ammonium phosphate when the precipitate is somewhat bulky. With small precipitates the influence of alcohol is not noticeable. There is, however, apart from the question of solubility, an advantage in the use of alcohol. The precipitate seems to be rendered more compact, is more easily washed, and is less liable to creep up the sides of the funnel. No advantage was found in adding alcohol to the solution in which the precipitation takes place. The precipitate, under these conditions, attaches itself closely to the beaker, and is not easy to remove. The filtration, also, seems to be retarded.

All of the precipitates were ignited in platinum crucibles, without removal from the filter paper. If the paper is carbonized, and burnt slowly with the crucible on its side, without the lid, there is no risk of injury to the platinum.

KIDDER LABORATORIES, MASS. INST. TECHNOLOGY; November, 1887.

*AN INVESTIGATION AS TO HOW TO TEST THE STRENGTH OF CEMENTS.**

BY JEROME SONDERICKER.

WITHIN the past year there have been designed in the Laboratory of Applied Mechanics of the Massachusetts Institute of Technology, apparatus for testing the compressive and tensile strength of cement which are believed to possess some valuable features; a description of the apparatus and some of the results obtained from them, will be presented in this paper.

COMPRESSION APPARATUS.

The compression apparatus was designed for testing not only cement, but also stone, iron, and other materials of construction. It was made to be used with an Olsen 50,000 lb. testing machine. A side view of it is shown in Fig. 1. The two pieces of steel, *A* and *B*, are fitted to the slot in the middle of the movable cross-head of the machine, and are bolted together by two $\frac{1}{2}$-inch bolts. These pieces receive the upper compression plate, *D*, which is secured to them by means of the center bolt, one inch in diameter. This plate is five inches in diameter. It is provided with a spherical bearing of two and one-half inches radius, the center of the spherical surface being at the center of the lower surface of the plate. The washer, *C*, is also provided with a spherical bearing surface concentric with the other, in order that it may have a firm seat in any position of the plate. The lower compression-plate is a cylinder one and one-half inches thick and five inches in diameter. It is doweled to the platform of the machine, its center being exactly under the center of the upper plate. Concentric circles are described on its upper surface, to admit of centering the specimen to be tested. Both plates are made of steel, their working surfaces being hardened and ground plane.

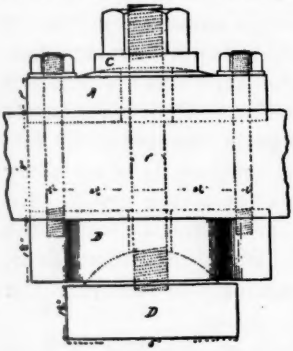


FIG. 1.

*A paper presented to the American Society of Mechanical Engineers, at their meeting of November, 1887.

Two requisites of apparatus for testing the compressive strength of specimens provided with plane bearing surfaces, are as follows:

1st. When adjusted in position, the plates shall bear uniformly over the whole surface of the specimen.

2d. During the progress of the test, the moving plate shall remain parallel to its first position.

These conditions were designed to be satisfied, as nearly as possible, in the apparatus just described by the following means:

1st. By providing the spherical bearing so as to made the upper plate adjustable.

2d. By making the radius of this bearing sufficiently large, so that the friction due to the pressure exerted, aided, if necessary, by tightening the nut of the center bolt, would be sufficient to prevent slipping from taking place, due to any eccentric pressure; such as would occur, for example, in the testing of a long iron specimen as a result of its side deflection when near the point of rupture.

In order to secure a uniform bearing over the surface of the specimen, the plate is suspended loosely in its socket; then, the specimen having been placed in position on the lower plate, the upper plate is brought down upon it slowly, at the same time being adjusted by hand approximately parallel to the surface of the specimen. As soon as the plate comes in contact with the specimen, it tips about its center until it comes to an even bearing.

In order to determine whether the friction of this spherical bearing would be sufficient to prevent slipping in ordinary testing operations in case the pressure should become eccentric, a wrought-iron specimen, five inches long and one inch in diameter, was tested. At intervals during the progress of the test, the distances between four points 90° apart, near the outer edges of the plates, were calipered with a micrometer caliper reading by graduation to thousandths of an inch, and by estimation to ten thousandths. By this means no slipping could be detected, even after the pressure had become eccentric to the amount of one-eighth of an inch, due to the side deflection of the specimen. This would correspond to a frictional resistance in the bearing of about .04 of the normal pressure. A second experiment was made to determine the amount of eccentricity which would just produce slipping in the joint when not oiled. A cylinder of iron, one inch in diameter, was submitted to pressure on its curved surface, and the eccentricity necessary to produce slipping under various loads was noted. The test was performed as follows: The plate was clamped in position, sufficiently firmly to prevent slipping under the eccentric load, by tightening the

nut on the center bolt. The specimen was then placed eccentrically in the machine, and subjected to various pressures up to 10,000 lbs. After applying the pressure the nut was loosened, thus allowing the friction of the ball joint to act alone in resisting the moment of the eccentric load. It was found by this means that, for all the loads tried, the plate began to slip when the eccentricity amounted to three-eighths of an inch. This would correspond to a coefficient of friction in the joint of about .12. No use has been made thus far of the center bolt, except for suspending the plate, and for this purpose, of course, it might have been made much smaller, together with the plate *A*. It is desirable, however, to have some means for fixing the plates parallel to each other, and also for providing for extremely eccentric pressures; the center bolt answers these purposes. It will be noticed that as the center of the spherical joint is in the center of the lower face of the plate, this point remains stationary in all positions of the plate, so that the pressure upon it is central however it may be inclined; also, the minimum amount of sliding between the plate and specimen occurs as the plate is coming to a bearing.

In testing a cube of cement, two opposite surfaces are rubbed down with water on a piece of slate, in order to remove any small surface irregularities. The load is at first applied rapidly up to a point well within the breaking load, and then slowly, and at a uniform rate of speed till fracture takes place. In order to ascertain the degree of uniformity of results to be obtained in the use of this apparatus, a number of tests have been made of two-inch cubes of both Portland and Rosendale cement, with and without sand, and from one week to three months old. Four cubes were made at one mixing, and all tested at the same time. Table I. gives the results of these tests, including the mean breaking load for each set of four specimens, the maximum variation of any one test from the mean, and the extreme variation occurring in each set. It will be noticed that the variations are quite small, the average maximum variation from the mean being 1.9 per cent, and the average extreme variation 3.0 per cent. No extraordinary precautions were employed in making these tests; they are believed to represent what may be expected in the ordinary use of the apparatus.

TENSION APPARATUS.

It is a well-known fact that the means commonly employed in determining the tensile strength of cement give results varying within wide limits, variations of 20 per cent, and even 30 per cent in the strength of specimens of neat Portland cement from the same mixing being

quite common, and this in the hands of careful experimenters. An examination of the report of Maclay's experiments, published in the Transactions of the American Society of Civil Engineers for 1877, shows extreme variations in the sets of five specimens of neat Portland cement, having a sectional area of $2\frac{1}{4}$ square inches, as high as 35 per cent, differences of 20 per cent and more occurring frequently. These wide variations are assignable partly to the impossibility of mixing the mortar and filling the molds so as to obtain briquettes of exactly the same strength, and partly to inexact methods of testing.

Experiments have been made in the laboratory with the view of reducing that portion of these variations resulting from the methods of testing employed. For this purpose clips have been designed, which are intended to eliminate eccentric pulls in applying the load.

Before giving a description of these clips, a few explanations will be made. The form of briquette in use in the laboratory is that shown in Fig. 2. The outline of the neck and bearing surfaces consists of circular arcs of one inch radius, terminated by straight lines tangent to them, and having a slope of 1 to 2 with the axis of the briquette. The sectional area at the neck is one square inch. The clips first in use had flat bearing surfaces having the same slope as the sides of the briquette. They came in contact with nearly the whole of the flat portion of the sides of the specimen, leaving a free space of about one inch between their points. Upon trial, it was found that quite a large percentage of fractures occurred at the edge of the clips. The following experiment was then made: Four narrow, thin strips of steel were inserted between the clips and briquette, so that the briquette bore upon these strips instead of upon the surface of the clips. It was found that as the strips were moved farther away from the neck of the specimen, the number of fractures occurring outside of the smallest section diminished, and at the same time the breaking strength increased, until, when the strips were placed two inches apart, nearly all the fractures occurred at the smallest section. Two inches was therefore selected for the distance between the bearing points of the clips for this form of briquette. The clips shown in Fig. 2 were then designed.

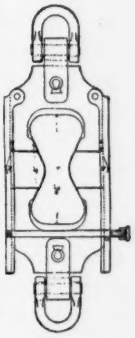


FIG. 2.

Two guides, grooved to a depth of $\frac{1}{8}$ inch, are hinged to the upper clip. The lower clip is secured in the guides by means of the clamp-screw *A*; the springs *B*, attached to the upper clip, release the lower clip from the guides when they are unclamped. A central pull is se-

cured by means of two pairs of knife-edges at right angles to each other, the outer pair being inserted in the yokes which bear on the inner ones. The yokes are prevented from moving sideways by means of projecting points bearing against the sides of the clips. The bearings of the yokes on the inner pair of knife-edges are notched, as shown in the figure; thus the yokes occupy a definite position. The outer knife-edges are adjusted so that their plane passes exactly through the axis of the clips, as are also the inner ones; thus the line of pull, which is determined by the intersection of the planes of the two pairs of knife-edges, is made to pass through the axis of the clips when these are clamped. The bearing surfaces of the clips upon the briquettes are rounded, forming blunt points.

These rounded bearing surfaces must be made accurately, and used intelligently, in order to secure reliable results. The points must be exactly central, and the bearing surfaces of the briquette square with its face; even then the adjustment of the specimen in the clips by hand in the ordinary manner, can only secure an approximately central pull, varying with the degree of care exercised by the operator and the condition of the bearing surfaces of the briquette. The object designed to be attained by the clips just described, is to secure a definitely central pull, at the same time making the task of adjusting the specimen in the clips as simple as possible. The operation of testing is as follows: —

In order to prevent the lower clip from dropping when the specimen breaks, a loosely-fitting brass band is slipped over the briquette before it is placed in the clips. The lower clip is raised sufficiently to allow the specimen to be placed in position, and clamped firmly enough to support its own weight. The specimen is inserted, and the lower clip drawn down till the bearing surfaces of the briquette are just in contact with it. The briquette is then adjusted so that its faces are flush with the faces of the clips. A load, usually about one half the breaking load, is rapidly applied. This forces the blunt points of the clips into the specimen, thus obtaining a firm bearing before the guides are removed. The guides are unclamped, the springs freeing them from contact with the lower clip. The stress is then applied slowly and at a uniform rate till fracture takes place. Before taking out the fractured specimen, the lower clip is clamped in position ready for another test. The only hand adjustment is in placing the specimen flush with the faces of the clips. The only possible eccentricity will be caused by the imbedded points not acting at the center of the specimen; if the points are themselves central and the sides of the briquette square, this will not occur. If, however, it does, it can be detected by the clips springing to one side

when the guides are unclamped. The grooves in the guides of the experimental apparatus are unnecessarily deep. With shallower grooves, admitting of more rapid clamping and unclamping of the guides, it is believed that the operation of testing may be performed fully as rapidly as by the usual method. The adjustment of the specimen in position is so simple a matter that the clips may be used by any careful workman with a certainty of securing reliable results. The knife-edge bearings answer the same purpose that is claimed for conical points, and are less liable to be dulled by use with heavy loads.

In order to determine the degree of uniformity in the results obtained by the use of this apparatus, 117 specimens have been tested. They include both Portland and Rosendale cement, with and without sand, and from seven to twenty-eight days old. The briquettes were made in sets of four, four molds being filled from one mixing. Three sets of specimens were made at the same time from three different mixings, using the same ingredients in all, and attempting to obtain the same quality of mortar. Table II. gives the results of these tests, including the mean strength of each set of briquettes, the maximum variation from this mean in pounds and per cents, and the extreme variation occurring in each set in pounds and per cents. The three sets of specimens made the same day with the same ingredients are grouped together and marked *a*, *b*, and *c*. Four of the specimens tested are excluded from the table; these are reported in the column of remarks, with the reasons for their exclusion. It will be seen from this table that the maximum variation from the mean is included between 0.6 per cent and 16.3 per cent, with an average of 5.7 per cent. The extreme variation in strength in each set is from 1.2 per cent to 25.6 per cent, with an average of 9.7 per cent. The cement was in each case unsifted, and the sand, ordinary coarse sand, sifted but not washed. The mixture was in each case of about the consistency of ordinary mortar, 25 per cent of water being used with the neat Portland, and $37\frac{1}{2}$ per cent with the neat Rosendale cement. It is believed that these results represent what may be expected in ordinary testing operations by the use of the clips described.

EFFECT OF AN ECCENTRIC LOAD.

In order to determine the effect of a slightly eccentric pull, the experiments recorded in Table III. were made. Briquettes of the same mixing are grouped together. In the sixth test, the briquette showed a large cavity near one edge of the fractured section. This, doubtless, decreased its strength considerably. The effect of such a cavity cannot

be estimated wholly by the area it occupies, as it also causes a non-uniform stress at the section. Briquette No. 8 was first subjected to a central stress of 640 lbs. without fracture. It was then replaced in the clips $\frac{1}{16}$ inch out of the center, and broke at 528 lbs., as recorded in the table. Omitting the second group, this experiment shows a decrease in strength due to a pull $\frac{1}{16}$ inch out of the axis of the briquette of 12 per cent for the first group, 17 per cent for the third and fourth, and 10 per cent and 9 per cent for the fifth and sixth groups respectively. The ordinary formula for eccentric loads would give a decrease of strength of 27 per cent. A greater number of tests would be necessary to determine the exact percentage of decrease of strength due to a slight eccentricity, but these serve to show the importance of guarding against eccentric stresses in cement testing, however slight they may be.

The machine used in making all the tensile tests was a single lever suspended in a wooden frame, the loads applied being indicated by a spring balance. This machine was tested at the conclusion of these experiments, and found to be correct.

The form of briquette used in making these tests is somewhat more slender at the neck, and considerably longer than that recommended by the committee on cement testing of the American Society of Civil Engineers. No comparative tests have been made to determine the relative value of the two forms. There are two qualities developed by testing which a cement briquette should possess. 1. The fracture should take place at the smallest section. 2. The breaking strength should be the maximum obtainable, the mortar being of ordinary stiffness. The latter requisite can only be determined definitely by comparative tests of different forms of briquette. In the tests just described, out of the 117 specimens tested, 88 specimens, or 75 per cent, broke at or within $\frac{1}{8}$ inch of the smallest section; 20 specimens, or 17 per cent, at a distance of about $\frac{1}{4}$ inch; 5 specimens, or $4\frac{1}{2}$ per cent, at about $\frac{3}{8}$ inch; 4 specimens, or $3\frac{1}{2}$ per cent, at about $\frac{1}{2}$ inch distance from this section. The areas of the cross sections of the briquettes at these points are, at $\frac{1}{8}$ inch, 1.015 sq. inches, at $\frac{1}{4}$ inch, 1.06 sq. inches, at $\frac{3}{8}$ inch, 1.15 sq. inches, and at $\frac{1}{2}$ inch, 1.26 sq. inches. None of the specimens broke within $\frac{1}{2}$ inch of the bearing points of the clips. Further investigation will be made in this direction.

TABLE I. COMPRESSION TESTS OF 2-INCH CUBES.

Cement.	Age.	Breaking load on 4 sq. in. Mean of 4 specs. Lbs.	Max. variation from Mean. Lbs.	Max. variation from Mean. %	Extreme variation. Lbs.	Extreme variation. %	Remarks.
Portland, neat.	95 d.	22,540	440	2.0	742	3.3	
" "	88 d.	18,981	181	1.0	270	1.4	
" "	77 d.	15,982	82	0.5	145	0.9	
" "	9 d. 6 h.	9,534	69	0.7	122	1.3	
" "	9 d. 1 h.	9,473	97	1.0	179	1.9	
" "	28 d.	21,614	101	0.5	180	0.9	
" "	14 d. 5 h.	17,867	242	1.3	375	2.1	
" "	13 d. 20 h.	16,469	269	1.6	450	2.7	
" "	7 d. 1 h.	10,061	164	1.6	225	2.2	
" 3 pts. sand.	14 d.	2,269	54	2.4	92	4.0	
Rosendale, neat.	76 d.	9,226	159	1.7	255	2.8	
" "	24 d.	4,210	310	7.4	470	11.1	
" 1 pt. sand.	14 d.	1,429	37	2.6	70	5.0	
			Mean . . .	1.9	. . .	3.0	

TABLE II. TENSION TESTS. (*Briquettes 1 sq. inch section.*)

Cement.	Age.	Breaking Load Mean of 4 specs. Lbs.	Max. var't'n from Mean. Lbs.	Max. var't'n from Mean. %	Extreme variation. Lbs.	Extreme variation. %	Remarks.
Portland, neat,	28 d.	(a) 616	28	4.5	52	8.4	
" "	28 d.	(b) 651	15	2.3	24	3.7	Mean of 3 specs. 4th broke in large cavity at 492 lbs.
" "	28 d.	(c) 620	26	4.2	50	8.0	Mean of 3 specs. 4th broke in large cavity at 558 lbs.
" "	14 d.	(a) 490	34	7.0	60	12.3	
" "	14 d.	(b) 523	55	10.5	82	15.7	
" "	14 d.	(c) 521	19	3.6	36	6.9	
" "	13 d. 19 h.	(a) 573	27	4.7	54	9.4	
" "	13 d. 19 h.	(b) 561	35	6.2	58	10.3	Mean of 3 specs. 4th placed eccentrically in clips and broke at 482 lbs.
" "	13 d. 19 h.	(c) 550	26	4.7	46	8.4	
" "	7 d.	(a) 322	8	2.5	13	4.0	
" "	7 d.	(b) 324	3	0.9	4	1.2	Mean of 3 specs 4th subjected to eccentric load and broke at 228 lbs.
" "	7 d.	(c) 312	2	0.6	4	1.3	
" 3 pts. sand.	14 d.	(a) 89	7	7.9	12	13.5	Mean of 3 specs. 4th broke in taking out of mold.
" "	14 d.	(b) 90	7	7.8	12	13.3	" " "
" "	14 d.	(c) 86	14	16.3	22	25.6	
Rosendale, neat.	24 d.	(a) 176	8	4.5	14	8.0	
" "	24 d.	(b) 170	10	5.9	20	11.8	
" "	24 d.	(c) 176	5	2.8	8	4.5	
" "	14 d. 4 h.	(a) 94	8	8.5	12	12.7	
" "	14 d. 4 h.	(b) 97	2	2.0	3	3.1	
" "	14 d. 4 h.	(c) 88	8	9.1	12	13.6	
" "	13 d. 19 h.	(a) 76	3	4.0	5	6.6	Mean of 3 specs. 4th broke accidentally.
" "	13 d. 19 h.	(b) 84	5	6.0	9	10.7	
" "	13 d. 19 h.	(c) 88	6	6.8	10	11.4	
" "	9 d.	(a) 82	2	2.5	4	5.0	
" "	9 d.	(b) 67	3	4.5	6	9.0	
" "	9 d.	(c) 67	5	7.5	10	15.0	
" 1 pt. sand.	14 d.	(a) 60	4	6.7	7	11.7	
" "	14 d.	(b) 59	4	6.7	7	11.7	
" "	14 d.	(c) 57	5	8.8	8	14.0	
		Mean . . .	5.7	. .	9.7		

TABLE III. EFFECT OF AN ECCENTRIC LOAD.

Cement.	No. of Test.	Age.	Breaking Load.	Remarks.
Portland, neat.	1	107 d.	508	Stress central.
	2	107 d.	516	Stress central.
	3	107 d.	444	Stress $\frac{1}{16}$ inch eccentric.
	4	107 d.	450	Stress $\frac{1}{16}$ inch eccentric.
Portland, neat.	5	23 d.	596	Stress central.
	6	23 d.	372*	Stress $\frac{1}{16}$ inch eccentric.
Portland, neat.	7	23 d.	632	Stress central.
	8	23 d.	528†	Stress $\frac{1}{16}$ inch eccentric.
Rosendale, neat.	9	24 d.	130	Stress central.
	10	24 d.	108	Stress $\frac{1}{16}$ inch eccentric.
Portland, neat.	11	12 d.	423	Stress central.
Means of 6 specimens.	12	12 d.	379	Stress $\frac{1}{16}$ inch eccentric.
Rosendale, neat.	13	11 d.	86	Stress central.
Means of 6 specimens.	14	11 d.	78	Stress $\frac{1}{16}$ inch eccentric.

* Broke in a large cavity near edge of fractured section.

† Subjected to a central stress of 640 lbs. This was then removed and the eccentric stress applied.